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The Nature of Learning

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The Nature of Learning

IN ITS RELATION TO THE LIVING SYSTEM

By

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ΣΩ. Ψυχῆς οὖν φύσιν ἀξίως λόγου κατανοῆσαι οἴει δυνατὸν εἶναι ἄνευ τῆς τοῦ ὅλου φύσεως;

ΦΑΙ. Εἰ μὲν οὖν Ἱπποκράτει γε τῷ τῶν Ἀσκληπιαδῶν δεῖ τι πείθεσθαι, οὐδὲ περὶ σώματος ἄνευ τῆς μεθόδου ταύτης.

The Phaedrus of Plato.

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THE PROBLEM

"In order to discover the mechanism of the living system, it is necessary to investigate which among its effects are connected with well-established laws of chemistry and physics and to distinguish them carefully from the effects which have no immediate, or at least known, relation with these laws, and of which the cause is concealed for us. It is these last that Vanhelmont and Stahl have attributed to a first principle or to the soul, without reflecting that, their nature not being plumbed, what they attributed to a single agent depended perhaps on many. In resorting to imaginary causes, does it not seem that these great men have tried to conceal their ignorance under the veil of philosophy, and that they could not make up their mind to indicate the limits of their positive knowledge? They are, without doubt, right in saying, and we agree with them, that certain phenomena are found only in organized bodies, and that a particular order of movement and of combinations forms the base and constitutes the character of these bodies. It would unquestionably be a mistake to assign to these phenomena hypothetical causes whose inadequacy has been shown. But however astonishing they may seem to us, these functions are surely physical effects, more or less complicated, whose nature we ought to investigate by all the means which observation and experiment place at our disposal, instead of supposing for them first principles on which the mind slumbers, in the belief that it has finished when it has not begun."

This passage is from the *Discourse on Anatomy* of Vicq

d'Azyr, a French physician writing in 1805.[1] It lays down a biological programme which has since been consistently carried out. One by one properties supposedly peculiar to the living organism have been shown to depend on principles common to organic and inorganic bodies. "Vital force", "animal heat", with many other once powerful words, survive only in the histories of science. It may, indeed, be said that the progress of biological thought during the years since Vicq d'Azyr wrote has largely consisted in the discovery of processes exhibited alike by living and non-living matter. Thus Claude Bernard maintains[2] in 1878 that "in reality there is only one physics, one chemistry, and one general mechanics into which all the phenomenal manifestations of nature enter, those of living as well as of non-living bodies," and this opinion is shared by most modern investigators. Indeed, for the majority of biologists the general principle underlying Bernard's statement has attained the dignity of an experimental postulate.[3]

When, however, we come to the organic activities with which the psychologist is mainly concerned, namely, those involving the organism's behaviour in relation to its total past history, it seems as though the postulate must be given up. True it is, of course, that every physical event is influenced by the total past history of the physical particles taking part in that event; the fire burns in the grate because of the changes induced by the sun's rays in the trees of the prehistoric forest, and because of the further changes produced in the process of formation of coal, and so on. But organic action introduces a principle which appears to be new, and unanticipated by any phenomenon observable in inorganic nature. For here the general dependence of events on preceding events acquires in general a *direction*,[4] namely that of the conservation of an organized system. Thus, to take an elementary example, it is clearly to the "advantage" of a sea-bird to learn that food may generally be found at a

[1] Vicq d'Azyr, 1805, vol. iv. p. 16. Quoted in the original by du Bois Reymond, *Reden*, 2, 27, and used by him as the motto of his monograph on Animal Electricity. Du Bois Reymond took the quotation from Humboldt.

[2] Claude Bernard, 1872, pp. 317, 319.

[3] See Needham, 1926, Ch. VI, for an able statement.

[4] The term is borrowed from Professor Köhler (1927 and 1930).

certain spot. Organic energy is saved by this knowledge, which thus tends towards the conservation of the material aggregation which we call the bird. Here the striking fact is not the dependence of the events of today upon those of yesterday, but the direction of that dependence, which is towards the conservation of the biological entity in question. When the bird dies, the fate of its constituent elements still depends upon their past history. If, for example, it is shot in mid-air and falls into the sea, the spot where it falls will depend on its position when it was shot. But the kinetic energy released by the fall will no longer work in the direction of the bird's conservation, as when during its lifetime it dives for a fish or flies to a steamer for food.

In order to explain these facts, certain special terms have been invented. Thus in the past it has been usual to speak of the "instinct" of Self-Preservation. Likewise it is fairly usual to speak of the organism as having "learned" where to find food or drink; or Modification of Behaviour is said to have occurred. Sometimes it is said that Memory of past events is present.

It is exactly here that we are in danger of falling into the pitfall of which Vicq d'Azyr warned his generation. Learning and Memory as applied to behaviour must not be assumed to be first principles, phenomena *sui generis*. In Vicq d'Azyr's words, "However astonishing they may seem to us, they are surely physical effects more or less complicated, whose nature we ought to investigate by all the means which observation and experiment place at our disposal." At the risk of repetition it must be insisted that it is not enough here to invoke the principle that the matter composing the organism is subject to the same physico-chemical laws as that composing non-living bodies. Suppose I say, for instance, that the gull flies straight to the fish wharf for food, because during the bird's past history certain physico-chemical changes have taken place in its nervous system. Such a statement has a certain scientific ring about it. It is perhaps preferable to the statement that, by a process of Trial and Error, learning has taken place. For it is precisely such terms as the latter that run the risk of being considered as explanatory first principles. To refer change of behaviour to physico-chemical change at least avoids these fresh and seductive possibilities of scientific slumber. But the feeling that an explanation

of learning has been given is illusory. For while it is true that the change in the animal's behaviour has presumably taken place in accordance with physico-chemical principles, yet this statement takes no account of the fact that this and thousands of other such sequences in the bird's lifetime may be observed to tend on the whole to the creature's advantage. To refer learning to physico-chemical change pure and simple is to omit the all-important characteristic of organic direction.

There is indeed another way in which we can take all the facts into account. We may maintain that the bird is a psycho-physical organism, and may claim that the fact that phenomena of a non-material order are occurring in connection with the organism explains the direction of the organic processes. Thus with MacDougall we may say that the bird exhibits the psychic phenomenon of purpose, and that the physical processes in the organism are thus psychically directed. With this interpretation many vitalists would probably agree. Now Professor MacDougall has performed a unique service for psychology in pointing out that organic action is directive. In his fight for the principle that the plain physico-chemical statement of animal and human behaviour is inadequate one must stand at his side. But in our efforts to carry out Vicq d'Azyr's programme in the description of learning, we should not, I feel, so easily give up the struggle. It seems, after all, to be a matter of intellectual consistency. Where psychology is making use of the methods and results of physiology, which in turn uses those of physical science, intellectual honesty seems to demand a rigorous insistence on the present presuppositions of physical science. This immediately excludes the possibility of taking into account any but "physical" facts, and banishes therefore any such *deus ex machina* as a directive psychic purpose.[1] It may conceivably be possible at some future time to say of organic processes: "Here physical science ends, here psychic influence begins." At present, certainly, it is not possible to draw this dividing line. Until that future time has come we have no right to speak of the organic body in the terms of physical science, while at the same time tacitly reserving the privilege, when we find ourselves in a difficulty, of postulating what must be regarded in the present state of knowledge as a miracle. When, therefore, the Plain Man tells us that it is

[1] That is, when we are dealing with behaviour.

perfectly clear that the mind influences the body, while admitting the cogency of his common sense, we must ask of him permission to shirk, for the time being, the difficulty he raises. Learning, purposing creatures are material complexes to which certain things are observably happening. Physical science has successfully developed a set of postulates and a technique for dealing with material phenomena. These postulates and this technique biologists and physiologists have adopted in the investigation of the particular material phenomena that interest them. That living matter apparently has connected with it a set of phenomena of a different order, namely those of consciousness, is as irrelevant to the investigator while he is using the method of physical science as are the chemical properties of the chessman to the chess-player.

We are left then with the original ancient dilemma. Living creatures are material aggregates, events in which several generations of workers in the biological sciences have most successfully analysed by the use of the postulates of the physical sciences and of techniques similar to those therein employed. Other material events, notably those in which psychologists are interested, seem, however, to require some qualitatively different principle of explanation or analysis. At the present stage of knowledge intellectual consistency seems nevertheless to require that when we are discussing the more complex actions of an organism we shall either reject entirely most of the findings of the newer biology, or attempt to find for the more complex organic events a method of description which will not contradict the basic principles on which biological science has been built. We cannot entertain the idea that there has been established within the living body a moratorium in the operation of elsewhere universal physico-chemical principles. It seems, for example, impossible to hold a theory of behaviour which assumes that the nervous impulse is a physico-chemical event, and which at the same time requires that some other than a physico-chemical event may initiate it.

It is no part of the purpose of this book to maintain that the programme indicated will or will not ultimately be carried out in its completeness. But, in the endeavour to follow up a modest part of it, the purpose of the succeeding chapters will be to show that organisms may reasonably be considered as material systems so constituted that organic events may

be truly subject to physico-chemical laws, and yet, on the whole, and under the requisite conditions, actively tend in an astonishingly complex manner towards the conservation of the living system in question. That is to say, an endeavour will be made to give, not indeed for the first time but perhaps somewhat more completely than before, a description of certain complex types of behaviour which at least does not in itself necessitate the hypothesis that special principles of explanation must be postulated for organic events. In these terms the problem will be re-stated without offering a detailed solution, which is in any case probably impossible at the present time. It will be seen that certain inorganic systems exhibit a property of self-conservation similar to that found in the organism. Making allowance for differences of complexity, this implies that the property of conservation is not unique to organic bodies. Further, the attempt will be made to exhibit learning as a specific instance of such self-conserving action.

Physical and biological literature apparently contain no discussion of the meaning of the word *system*. The general implications of this term will accordingly be considered, following which there will be discussed the implications of the special term *living system*. Finally, the concept of " learning " will be examined.

CHAPTER II

THE CONCEPT OF SYSTEM

"But when there is some contradiction always present, and one is the reverse of one and involves the conception of plurality, then thought begins to be aroused within." (Plato's *Republic*. Jowett.)

In order to obtain a clearer insight into what is meant by a "living system" it will be profitable first to examine the meaning and implications of the term system. This will imply an examination of this term as it is used in connection with inorganic phenomena. At the outset a caution should be issued. It should not be imagined that, by the use of such a term in description of both inorganic and organic events, any attempt is being made to give a final explanation of the latter. It will appear in the sequel that living and non-living complexes have certain properties in common. But this has been seen to be no new step in the history of knowledge; for it has already been pointed out that the history of biological science in the nineteenth and twentieth centuries has largely consisted in the discovery of exactly such common properties. Popular thought has long recognized certain elementary similarities between living and non-living substances. Inertia, solidity, and weight clearly belong equally to living creatures and to inorganic matter. From the tacit recognition of such simple properties as these it is a large step to such works as those of Bayliss on *Interfacial Forces and Phenomena in Physiology* and Thompson on *Growth and Form*, each of which applies very refined physical analysis to bodily events.[1] But the step is in the same direction, although in neither case does it take us to the end of the path. A man left unsupported at the top of a cliff will fall to the ground under the same gravitational laws as the inert matter of the cliff. Nobody would claim this fact as a final explanation of human nature, although as a description of

[1] See the References at end of volume.

one aspect of the man's body it does throw a certain light upon him. *Per contra*, it was by the fall of an organic body that Newton is said to have been stirred to the investigations which brought him to certain conclusions concerning matter in general, both organic and inorganic. If the apple story is true, Newton certainly did not care whether the suggestive incident involved living or non-living substance, and he most assuredly did not feel that his discovery gave a final explanation of the fruit. At the risk then of repetition it must here be stated that in discussing certain properties of inorganic matter with reference to their similarity to those of living substance, the presumptuous attempt is, of course, not being made to give a final explanation or description of life or living function. Such comparison as will be made merely goes one step farther along the path already taken by common observation and scientific research alike. Final description or ultimate explanation may well wait the scientific day of judgment.

The Definition of "System".—A system may be broadly defined as a complex or aggregate of elements which are considered to be connected by a special set of relations. By means of their interrelations the logically distinct elements are combined into one, so that the complex exhibits the paradox of the many-in-one which interested Plato. The system is thus a whole of parts, the wholeness being given by the relations existing between the constituent elements. "All relations", says Bradley (1930, p. 521), "imply a whole to which the terms contribute". The system is just such an implied whole, within which may be distinguished related terms.[1]

The above definition is substantially the same as that given by Bishop Butler, the moralist. "A nature is an integer", he says, "its parts having reciprocal relations needful to be known. . . . Whoever thinks it worth while to consider this matter thoroughly, should begin with stating to himself

[1] Bradley adds "and by which the terms are qualified", thus implying his belief in the "internal relation". The converse proposition, that every whole must necessarily imply related and distinct entities as parts, is perhaps open to question. Miss Stebbing makes certain statements about the kind of elements that may be included in a system (1930, Ch. XI). They must be compatible and must all be determined by some at least of the others.

exactly the idea of a system, economy or constitution of any particular nature or particular anything, and he will, I suppose, find that it is a one or whole made up of several parts; but yet, that the several parts even considered as a whole do not complete the idea, unless in the notion of a whole you include the relations and respects which those parts have to each other. Every work both of nature and art is a system" (Butler, *Sermons*, Preface, 1726; in Gladstone, 1897). There should thus be distinguished first the elements of the system, then the relations by which these are inter-connected, and lastly the totality which is constituted of the elements in relation. Examples may be given. The watch is clearly a whole, and was given by Butler as an example, though the use which he made of the example is perhaps questionable. The watch is one watch, though it comprises a large number of separable wheels and other "parts". The total complex is these parts in certain specific relations and it is these relations that make a unity out of what would otherwise be a minor junk heap. Again, the British Constitution is a system. It may be considered as a "community of persons with a machinery for self-expression through a legislature, by which self-expression, called the law, and by it alone, authority to control the actions of individuals is conferred" (Bosanquet, 1920, p. 72). Here the elements of the complex are the persons composing the group; these persons are interrelated by the law, and by means of this interrelation a unity, a whole, is brought into being out of logically separable entities.

The distinction between the totality and its elements is well put by Aristotle who speaks of "that which is compounded out of something so that the whole is one, not like a heap but like a syllable,—now the syllable is not its elements, *ba* is not the same as *b* and *a*, nor is flesh fire and earth (for when these are separated the wholes, *i.e.* the flesh and the syllable, no longer exist, but the elements of the syllable exist and so do fire and earth); the syllable then is something,—not only its elements (the vowel and the consonant) but also something else." Although the example of the union of fire and earth to make flesh is perhaps a little unfortunate, yet the distinction between the whole and the elements is here perfectly clear, although many would hardly care to subscribe to all the implications of the rest of the argument. (Aristotle, *Metaphysics*, 1041b, 12. Tr. Ed. Ross.)

Inside and Outside.—Clearly a system as thus defined has in general a definite "inside" and "outside". The particular interrelated elements in question are thought of as inside the complex, everything else as outside it. It is true, as will be seen later, that such elements may also be related to objects external to the system ; but by definition these latter relations are not part of the special set of relations connecting the constituent parts of the system. For example, every part of the watch is related to the surrounding atmosphere. But this is something different from the relations obtaining between the parts of the watch and which constitute the whole collection of springs, wheels, etc., into an apparatus by which the time may be told. It is true that in considering the watch as a separate system we are to a certain extent simplifying our data. We are considering the watch-parts as related to each other, and shutting our eyes to the fact that they are also related to the surrounding conditions. But the simplification is a justifiable and indeed a necessary one, if we are to be able to deal with the world at all. To this point we shall return later. Meanwhile, it is pointed out that when the statement is made that the relations connecting the elements of the system are special or unique, the implication is that there are other portions of the universe that are not part of this connected whole. The whole universe is thus divided into the system and the rest of the universe, that is to say into what is inside and what is outside the system in question. This distinction is then inherent in the definition. The Zulu or the Hottentot is external to the British constitution. Although these are human beings, yet they do not enter into the peculiar relationships which make up the British constitution ; they are the "lesser breeds without the law".

The Dynamical System.—Now what has been said applies equally well whatever be the peculiar nature of the elements and their relations. Examples have been given where these relationships were of a mechanical and of a legal nature, giving what are called mechanical and legal systems respectively. Such relationships may, however, be of very great variety. We may have biological systems, astronomical systems, chemical systems, geometrical systems and so on, and in each of these cases the general rule holds that by means of a special set of relations a number of logically

separable elements are connected up into a unity. These different systems are of course investigated by what are in present-day practice different sciences; whether in the fulfilment of knowledge these will ultimately prove to be the same science, it is no part of this discussion to enquire. At least, however, in the present stage of knowledge it is clear that, starting with the general concept of system, we may, by substituting particular elements and relationships, obtain specific types of system such as are actually dealt with by specific sciences at the present time. Certain branches of knowledge have indeed made liberal use of the concept and have found that it presents a highly valuable method of description. Some of these cases will now be considered.

Before, however, examining the use which has been made of the concept of system in particular sciences, it is well to remind ourselves of our chief interest, namely the organic body. It will be our purpose in the next chapter to consider the organism as a living or biological system. Herein we shall be doing no more than exhibit a complex with a peculiar kind of organization or set of relationships, parallel to, but not deducible from, the chemical system, the legal system, the astronomical system and so on. Once more, in thus exhibiting the organism as a living system we are not attempting to "deduce" human behaviour from physics or chemistry, any more than Bosanquet attempts to deduce the British Constitution from an electrical installation when he suggests that both are systems.

An excellent example of a specific kind of system, whose definition may be obtained from that already given by substituting specific elements and relations, is the dynamical system. "Any material system is regarded from the dynamical point of view as constituted of a number of particles subject to inter-connections and constraints of various kinds, a rigid body being regarded as a collection of particles which are kept at invariable distances from each other by means of suitable internal reactions" (Whittaker, 1904). In this definition two points are stressed. First of all, the system is a complex. It comprises a number of elements. Secondly, these elements are not entirely discrete, but are interrelated so that they form the unity which is the system. The system is the inter-connected "whole" of which the elements are the

"parts".[1] Mathematical analysis determines and expresses in a symbolic form the properties of the parts of such systems as contrasted with the whole, and the properties of the whole as contrasted with those of the parts. Thus a gravitational pendulum swinging in air from a support forms a simple mechanical system, and it is possible to determine the rate at which the centre of gravity moves, this being a property of the whole. Or there may be determined the maximal tension of the supporting rod which is a property of a part.

Further, the system has a definite "inside" and "outside". The word internal is specifically used with reference to the special case of the rigid body.[2] A blow from a body external to the pendulum may cause it to swing. Here work has been done on the system by the external agent, and unless we are speaking of the total universe, it will always be possible to find such an external agent which may do work on any particular dynamical system. Every dynamical system with the exception of the total cosmos must then have an Outside. The system and the rest of the world are inseparable in practice, but are logically distinguishable, like Aristotle's excellent illustration of the concave and convex side of a curve.

It is perhaps well here to point out specifically that the mathematical treatment of such a mechanical system as a pendulum deals of course with an ideal case, where such abstraction and simplification is made from the facts as suits the purpose of the analysis. Thus the postulate is often made that the system is frictionless, in which case the pendulum, once started, moves for ever, providing an example of what is called reversible motion. In practice, however, as Köhler has pointed out (1927) there always does exist some force analogous to friction, which brings this and similar systems to rest.

Such checking processes may of course be taken into account in the mathematical calculation, but, even so, mathematics as applied to physics can at best treat of an ideal and simplified world only. Sufficiently close approximation can be made

[1] The word "part" is used without any further implication.

[2] The term *internal* is used also by Hertz (1899, 117); the distinction between external and internal is implied by Maxwell (1910), and indeed by all dynamical treatments.

for all practical purposes, but it is important to remember that mathematical physics involves a high degree of abstraction and simplification, and that it may conceivably turn out that the factors ordinarily neglected as immaterial for the purpose of mathematical analysis may be of high importance in the description of biological events. "The whole process of scientific investigation, *i.e.* the process of analysis, starts with partially destroying the natural 'together'[1] and replacing it by an artificial arrangement such as never exists in nature itself. To take an example—the chemist starts with preparing pure substances, such as are got for instance by crystallization, or which in some other way are capable of preserving some stability, some sort of independent existence. Such substances do not really exist in that isolation in nature. The things therefore which the chemist investigates are thus artificial in as much as they do not occur in nature in that isolation, stability or purity which are the necessary qualities of substances analysed or produced in the laboratory" (Merz, 1910). It may be added that they never occur even in the laboratory with these absolute qualities, such as theoretical treatment demands. To this point it will often be necessary to recur.

Thus the mechanical system as treated mathematically illustrates the whole-part property of the system in general, and exhibits the fact that mechanical relations are necessary to combine the elements into a mechanical unity. It illustrates also the separateness of the system from the rest of the "external" world, a separateness which, it has been noted, is achieved through a greater or less degree of simplification, but which is, none the less, intellectually justified and indeed necessary. Similar illustrations could be made of many other types of material system and will in fact be made implicitly in the sequel. Hereafter by system will be understood a material system, of the type treated by physical or biological science.

The Partial System.—A system may contain within itself another system. The second, sub- or partial system may for example consist of a group of particles bound by a set of inter-connections in which the rest of the system does not share. Such, for example, would be the liquid in a thermometer, which is ordinarily in a different material state or

[1] "Ensemble."

phase from the rest of the apparatus; or the spring in a spring balance, which, as relatively compressible, may again be considered to be composed of parts in different mutual relations from those holding together the weighing platform or hook. Such special interrelationship is, however, not necessary; any selection of the total related elements may be considered as together forming a partial system.[1] The possibilities of relationship between the whole, "free", system and the partial system are very numerous and the general problem has apparently not yet been treated. It may be that general treatment is impossible. At any rate, the question of the whole within a whole is one of the most difficult of the many difficult problems raised by the whole-part conception.

Certain special cases have been subjected to mathematical analysis. Thus Hertz (1899) deals with two cases of the mechanical partial system. Willard Gibbs deals with certain cases in his discussion of "heterogeneous systems", those composed of different forms of matter, and is able to come to important conclusions concerning the total relations of the different systems. Thus the "Phase rule" makes a statement concerning the number of "phases", solid, liquid or gaseous, which can co-exist among the different substances forming such a system. The achievement of such a rule with a total disregard of the means by which the necessary changes may be effected may be considered as one of the outstanding products of the human mind. It is a striking example of the treatment of systemic properties without consideration of the internal mechanism of the system.

The general conception of the partial system is important for the description of the biological system, a fact which makes all the more regrettable the apparent lack of extant general treatment of this complex subject. The following elementary considerations are advanced with some diffidence, in the hope that they may be replaced by a more adequate analysis.

When a material system is described as free, total, or isolated, the elements are considered to be related to each other and to nothing else. Thus Willard Gibbs defines a system as any portion of matter considered as cut off from the rest of the universe, and, in order to satisfy what may

[1] An exception may conceivably occur in a possible case where two or more elements are related to a further element or set of elements without being related to each other.

be called practical conscience, supposes the system to be surrounded by an impervious envelope. There may of course be change within such a free or total system, provided that such process is not inconsistent with the fundamental unifying relations. If any process runs counter to these fundamental relations, then the system is destroyed. Thus in these constituent relations is provided in mathematico-physical theory that permanence which philosophical thought has long insisted must underlie all change.

For example, the position of the tides will be in invariable relation to that of the moon, neglecting, that is to say, the influence of other factors such as the sun, the earth's rotation, and so on.[1] The system earth-water-moon may thus be described in terms of invariable relations, such as it is the express object of mathematical analysis to exhibit. Change indeed there is in the system, but the fundamental relations remain thereby unaltered; and for exactly that reason we are able to say that we have to do with the same system before and after the process of change. If, however, attention is confined to the partial system earth-water, neglecting the third member of the triad, it will be necessary to give a number of different and apparently unrelated descriptions of the water on the earth. In fact, these descriptions, taken unsimplified, will give what would, if the moon were not considered at all, have to be considered a number of different earth-water systems. For the relations of position between an oblate spheroid and the water on its surface are different according to the locus of the water, so that for each such locus a different system is constituted. By introducing the moon, however, a constant set of relations for the totality earth-water-moon may be exhibited, and the whole complex may be considered to be the same system at different times and in different positions.

The general principle is easily seen. If in the total system alterations are taking place such that a permanent basis exists for the change in question, there is no guarantee that within the partial system any such permanent basis exists for the alterations there occurring.[2] To consider any sub-

[1] That is to say, adopting also the general principle of simplification of observational data.

[2] See, however, Hertz, *loc. cit.*, for a special case where a partial mechanical system may be described *per se* in constant terms.

group (earth-water) as a partial system is for the time being to refuse to take into account certain elements (moon) of the total system and their relations (*e.g.* gravitation) to the rest of the system. This will often involve the refusal also to take into account the relations ("laws" governing the tides) whose constancy secures the persistence of the total system. Thus there will occur in the partial system processes which can be referred to no underlying law, and which involve the partial system alone. In the strictest sense of the term, the elements under consideration will, in respect of these processes, constitute different systems at different times. Considered strictly in itself, then, the partial system does not persist; it gains, however, a certain derived permanence from the constant relations which exist between its elements and those which, with them, comprise the total system. Thus the term partial system is justified.

The above statement applies to the general case of the partial system which is comprised of a random selection of elements from the total system. More generally, however, the partial system corresponds to a more or less robust natural articulation. That is to say, there exists a special set of more or less permanent [1] connections uniting certain elements of the total system and not shared by the other elements, and these specially connected elements are treated as a partial system. For example, in the case taken the tides are in physical contact with the earth and remain so in spite of ordinary change of the moon's position. In the ordinary thermometer the height of the mercury column varies with the outside temperature. At different times, then, and taken strictly in themselves, these varying columns of mercury must be considered to form different dynamical systems, having, for example, different centres of gravity; although because of their constant relationship with the external temperature they may be considered to be the same changing partial system of the total thermometer-air complex. Apart, however, from such permanency the fluid both before and after the change of temperature may be considered as comprised of a certain number of mercury molecules in the liquid phase.[2] The special set of relations, whatever they are, that unites

[1] See below for a discussion of such permanence in spite of processes making for change.

[2] The liquid-vapour equilibrium is here neglected.

these molecules, forms a natural articulation in the whole system air-glass-mercury, and this set of relations is ordinarily robust enough to persist in spite of changes in the rest of the system. If, however, these latter changes pass a certain limit this special set of relations no longer persists; the mercury boils or freezes. Thus in the mercury of a thermometer we have a partial system that, in addition to the permanence it gains by relation with the total system, also has, in itself, a measure of constancy due to its own internal organization. The same thing is true of the spring of a spring balance. One may say that regarded in a simplified way as systems in themselves these complexes are constant, but they have aspects [1] that are variable. By a process of simplification we may say that the mercury comprises the same number of molecules before and after a change in temperature,[2] and leave our description at that. If, however, we go further with the description and refer to the height of the column, then an element of variability is introduced.

The events occurring in such a partial system may be said to have two sets of determining conditions. They will be determined both by the special relationships of the partial system, and also by a set of relationships which the elements comprising the latter bear to the total system. Such partial systems with a more or less permanent organization of their own are of common occurrence in biological structures.

In general, when any partial system is described in and by itself certain relevant factors are being left out of account, namely those that spring from the relationship between the elements of the partial system and the rest of the total system. By contrast with the constant total system this gives to the partial system a certain capriciousness or variability. The description of a complex as a partial system is indeterminate and incomplete. It is, however, often a convenient, and sometimes indeed the only possible, method of description. This is the case when the rest of the system is more or less unknown, but is assumed to exist and to bear a fixed relation to what is known. Indeed, the variability of the partial system comes from the fact that we have sacrificed the invariability of description that comes from taking all the factors into account, and we have made this sacrifice either

[1] Sometimes called parameters.

[2] See note, p. 16.

because we must, or because we wish to concentrate attention on the partial system as such.[1] Description by means of the concept of partial system is then often a compromise with ignorance. For it is the assumption of all science that if we knew the total conditions then we should be able to show that the apparently capricious variations of any given set of phenomena conform to invariable laws or relations.[2] Such is the case, for example, with the proverbially inconstant atmospheric variations constituting the "weather", which, however, we do not nowadays regard as the sport of a malevolent deity who momentarily creates fresh—and generally pernicious—weather systems, but as ultimately referable to the invariable conditions governing a system more comprehensive than the one we are able directly to observe—perhaps, in the end one including the electrical changes of the Sun.

This concept of partial system is in practice the necessary complement of that of the whole or "free" system, for it is in practice never possible to make a complete description of any concrete thing; there always remains a residual variable factor or factors affecting the state of what is actually observed. It has been seen that this variability does not, however, invalidate the description of these aggregates as systems, for they are complexes of interrelated elements, although they are treated as also related to the external world. Further, the partial system is in a sense subordinate to the whole or free system of which it forms part, though by its systemic unity it achieves a certain measure of independence[3] within the whole.

This relationship between the total and the partial mechanical system is analysed by Hertz (1899) for certain cases. Hertz defines a connection of a (mechanical) system as normal when it is independent of the time. A material system subject to no other than internal and normal connections is called a free system. If the connections of a

[1] When the total conditions are known, the partial system is what Vaihinger calls a fiction. We think of the earth-tide system for the moment as if it were isolated.

[2] I do not feel that we can as yet deal with the principle of Indeterminacy in this context.

[3] It has been seen that this may be an ideal independence only, as when half a rod is regarded as a partial system, the half implying, as Aristotle said, the whole.

system are different at two different times, then "for the present we must consider that we are dealing with two different systems" (p. 80, section 120), the words "for the present" referring to his first book where only free systems are considered. "Every unfree system we conceive to be a portion of a more extended free system. . . . If we wish to emphasize this relation we shall denote the unfree system as a partial system, and the free system of which it forms a part as the complete system" (section 429, book two). "When a part of a free system is considered an unfree system it is assumed that the rest of the system is more or less unknown. . . . This deficiency of knowledge must in some way be met by special data." Hertz then analyses two cases, the first, perhaps of the more general interest, being that of the guided system, which undergoes motions that are performed "while the other masses perform a determinate and prescribed motion." He shows that the "equations of condition of a guided system contain in general the time," that is to say, again, cannot be expressed in such a form that they are the same at all times. Such variation, in the case of a free system, would make it necessary for us to "consider that we are dealing with two different systems." As related to the more inclusive system, of which certain facts only are known, a partial system here gains a continued identity, which it would lack if it were not so related. Thus it may be said that such a mechanical system is allowed more variation than can be accorded to a free or complete system. Although Hertz confines his treatment to mechanical systems, the logic of the discussion is the same whatever kind of system is being considered. What he gives appears to be a special analysis, in special mechanical terms, and with special mechanical assumptions.

Much the same general point is also raised by Bosanquet (1920) in his masterly little treatise entitled *Implication and Linear Inference*. For Bosanquet inference, that is to say, the extension of knowledge, is effected by the exhibition of the implications of a system. If we fully understand the system we are forced to certain conclusions, to deny which would be to deny the whole of knowledge. Some of the facts are directly observed as data, others are "arrived at primarily as conclusions or inferences from the system. . . . As data,

vouched for, that is to say, simply by evidence, they can possess neither full meaning nor full certainty. For, for all the evidence can tell us, they may be so today and not tomorrow." When, however, they are shown in their full systemic relation, they lose this uncertainty. Now this seems to be very much the same argument as that of Hertz, though it is expressed in general rather than mechanical terms. Hertz' partial mechanical systems are assumed to belong to more extended free systems, which are, however, not completely known. The special case of the guided system assumes, however, a certain specific knowledge of the changes taking place in the unknown parts. The reason why these partial systems are considered at all is that they are given by observation, while the rest of the total system to which they belong is incompletely given. Consequently, because they cannot be exhibited in their full systemic connections, the partial systems " possess neither full meaning nor full certainty. For all the evidence can tell us, they may be so today and not tomorrow." They may vary from time to time, or in Hertz' language, " their equations of condition contain in general the time." This agreement between the logician and the mathematician, each of them, it may be mentioned, an iconoclast in his own field, seems to provide an excellent illustration of the fundamental interrelation of logic and science. The reader is referred to the original passage in Bosanquet, where the point is illustrated most excellently by an account of Harvey's discovery of the circulation of the blood.[1]

Every System strictly speaking a Partial System.—It may now be seen that full description would make every material system strictly speaking a partial system, except the universe itself, the " system of the physical world " (Stebbing). It is equally true of dynamical and of geometrical analysis that it applies only by abstraction and simplification to the data of experience. Only by abstraction can we speak of a pendulum without the earth, of the earth without other gravitational bodies, and so on. " There is not in nature," says Poincaré (1913, p. 102), " any system *perfectly* isolated, perfectly removed from all external action ; but there are systems *almost* isolated." So that it is now possible to give a fuller

[1] I am indebted for this reference to Professor Reid MacCallum of the University of Toronto.

meaning to the statement that it is only by abstraction that we can consider any complex as a partial system. For the same statement applies to all systems of experience, whether for the specific purpose of the moment they are considered to be partial or free.

The Property of Constancy.—It has been seen that a system must exhibit some sort of constancy in order that it may be recognizable as a system from instant to instant. In the systems previously considered, which are the products of a high degree of abstraction and simplification, such constancy is ultimately [1] given by the assumption of the persistence in the system of identical material elements and relations. In the older physics a fundamental permanent in change was given by two postulates which seemed empirically justified, namely those of the conservation of mass and energy—propositions which, it is worth noting, applied to the system as a whole. While the newer physics has apparently shaken these laws at least in the form in which they were originally expressed, yet it has inevitably provided a substitute to ensure the necessary constancy. " The suggestion is that in almost any conceivable world *something* will be conserved. Mathematics gives us the means of constructing a variety of mathematical expressions having this property of conservation " (Bertrand Russell, 1930). But there are, however, to be found in inorganic nature cases of a somewhat less transcendental constancy, cases where the interrelations of a system are such that a threatened change in some particular respect is followed automatically by compensatory changes tending to keep constant the threatened factor. Let us first take such a simple mechanical system as the common gravitational pendulum. If physical force is applied to any part of the pendulum, provided that a certain limit is not exceeded the pendulum is not disrupted. This is due to the fact that intermolecular forces in the material of which the pendulum is composed offer a compensatory resistance when the external force is applied. Were the pendulum rod of sand, or of charred thread, it might hold together for a moment but the limit of resistance would be much smaller. Were the relations of its particles those of a gas, there could be no pendulum save perhaps of an astronomical nature. The existence of the pendulum system depends upon the mutual relations of the

[1] I am unacquainted with the theory of the " dissipative system ".

particles composing it, and in this elementary respect, namely that of offering a resistance to external change, it has been seen that any solid body may be considered as a system. But the pendulum is capable of maintaining another constant, namely a vertical position with reference to the surface of the earth. If the bob is struck, it settles down again after a series of oscillations with the thread in the same position as before the blow. The particles composing the system, the " elements " of the complex, are so organized that the vertical position is, in a constant gravitational field, restored after external disturbance. Many similar examples could be given.

The pendulum is a dynamical system. We consider it as obeying the laws of mechanics, as responding with movements of the whole complex to external disturbance of a mechanical nature and as being brought to rest by frictional agencies, which may be treated mechanically. Let us now take what is known as a statistical system, which is one the elements of which cannot be treated individually but only collectively. Such are the chemical systems, where the interrelations of the elements are considered to be of a chemical nature, and where the chemical reaction is considered as a whole, apart from what may be called the chemical fate of any individual molecule or atom. Here it will be noticed that the whole property of the system is exhibited *par excellence*. It is indeed only because the chemical reaction can be treated as a whole that we are able to describe it at all. This is excellently illustrated by what are called the reversible chemical reactions. For example, when it is heated to a high temperature phosphorus pentachloride splits up into the trichloride and chlorine. At the same temperature the latter two substances also combine to form the pentachloride. At any given high temperature, there is a certain percentage of the pentachloride left undissociated at any moment. At 200° 51·5 per cent of the pentachloride is left, the remainder having split up. At 250° these proportions are 20 per cent and 80 per cent, and so on. But this does not mean that at these temperatures 51·5 per cent of the pentachloride molecules are permanently united. The whole system is in a state of flux ; at any moment, for example, an individual atom of chlorine may be free, while at the next moment it may form part of a pentachloride molecule. But at any given temperature there are always in the aggregate approximately so many trichloride molecules

and so on. Thus a specific number of such molecules at a given temperature will exhibit a constancy which essentially involves change of individual elements, but which is nevertheless none the less real. Let us say that such a chemical system is, in an elementary way, so organized that there will always be present in the average a certain specific number of pentachloride molecules. This conserved group will be composed of different molecules from time to time; but there will always be present such a group, as long as the system persists. And of course under the highly idealized conditions assumed, where the temperature, pressure, number of molecules and so on are considered as constant, the system is immortal.

Such ideal conditions are, of course, not realized in practice. In practice there always occurs in the surroundings some change with reference to which such a system is always in reality a partial system. It is interesting to observe what happens when such a change does occur. Such a reversible chemical system obeys what is called the Le Chatelier-Braun rule, which is thus stated, "If a change occurs in one of the factors determining a condition of equilibrium, the equilibrium shifts in such a way as to tend to annul the effect of the change" [1] (Edgar, 1924, p. 307). A different example will be given. If water vapour is heated to a high temperature, decomposition takes place into the constituent gases, hydrogen and oxygen. But at such temperatures the reaction is reversible, these two gases combining to form water vapour. At any particular temperature the two opposing processes will come to a position of equilibrium, so that there is always a certain proportion of the molecules combined as water molecules, and a certain proportion existing as free molecules of hydrogen and oxygen, as in the case of reversible action already considered. If the temperature of the system changes, then this equilibrium is disturbed and a fresh one is taken up. Le Chatelier's theory states that when such a change of temperature takes place the system, in passing from one state of equilibrium to the other, gives out heat if the temperature is lowered, and absorbs heat if it is raised. Thus its action is in this respect compensatory for the disturbance

[1] There are certain restrictions to this statement. The factors are three in number, viz., heat, electrical or mechanical energy. The equilibrium must be stable. See Lotka, A. J., 1925, chapter on Le Chatelier's Rule.

of the equilibrium. The system appears to " resist " change. Similarly for the pressure and electrical conditions. Many other examples could be given; the rule is, with certain restrictions, universally valid, and it may be deduced from the laws of thermodynamics.

Here we seem to have, so to speak, a superstructure built upon the fundamental principles of physical science. The laws of thermodynamics are of course of absolutely general validity. They apply to matter and energy, however these may be naturally or artificially distributed. But given certain additional conditions of organization and state, then these laws may operate in such a way that they tend to produce an additional result, in this case a new " conservation " effect, which is not that of mass or energy but of the temperature of an aggregate of molecules. It is important here to realize that it is, again, only by a process of mental abstraction and simplification that we are able to obtain a complex which obeys Le Chatelier's rule in its clear-cut theoretical form. For this is an ideal result, depending upon conditions which are in practice never realized. Ideally we are able to keep all other factors of the equilibrium constant. Practically we are never able to do so with *absolute* exactness. In nature, one would suspect that even rough obedience to the rule is excessively rare. In the laboratory, it is possible to obtain an aggregate that will furnish a very close approximation.

Here, then, is a system in a chemical equilibrium, or constant state, which involves momentary change of individual molecules but in which are preserved the number of the molecules of certain specific kinds. When external conditions alter, this constant state is disturbed to be replaced by a new one, the transition having to a certain extent the result of compensating for the direct alteration which would otherwise be brought about by the external change. Change of chemical equilibrium tends to compensate for a change of temperature and so on. It may be said that the chemical complex tends to maintain a constant pattern or structure of an elementary kind, a pattern in which different individual molecules may be involved from time to time, but which, nevertheless, tends to preserve its constancy as a pattern.

Such constancy of pattern with individually changing elements, and ultimately perhaps depending on the same principles, is seen still more strikingly in what are called the

buffer solutions, which have been used so extensively by biologists for the last fifteen years. A buffer solution is one which tends automatically to stabilize its acidity or alkalinity. This means that in any given solution the concentration of hydrogen ions and of hydroxyl [1] ions is maintained constant. As an example may be taken an experiment of Henderson's quoted by Bayliss. The reader may be reminded that carbon dioxide functions as a weak acid. "We take a solution of 1 kg. of sodium bicarbonate in 100 litres of water and allow it to attain equilibrium with an unlimited atmosphere containing one gramme of carbon dioxide per litre. Let hydrochloric acid be added in small portions at a time, constantly shaking the solution so that there shall always be equilibrium with the carbon dioxide in the gas phase." . . . When this is done a characteristic thing happens. The hydrochloric acid reacts with the sodium bicarbonate, with the formation of sodium chloride and the liberation of carbon dioxide. This cannot be retained in the solution, for the carbon dioxide there is already in equilibrium with that in the "atmosphere". Consequently the newly liberated carbon dioxide escapes from the liquid, and the acidity remains what it was before, the only effect being a diminished concentration of the alkaline salt. When, however, all the bicarbonate is neutralized, this "buffer" effect ceases to operate. At this stage the addition of two grammes of hydrochloric acid "causes nearly as much rise in acidity as the previous 318 grammes had done, or about two hundred times the rise caused by a hundred times the amount at the first stage of the experiment" [2] (Bayliss, 1924, pp. 200, 201).

Using the same metaphor as before, here is a chemical system which maintains the same elementary pattern, namely, the concentration of hydrogen and hydroxyl ions. The solution system is partial to that comprising both it and the atmosphere of carbon dioxide, while this large system is again strictly partial to one containing the acid also. Considering the system comprising the solution and the atmosphere it may be said to be so constituted that by means of compensatory systemic changes a certain constancy is preserved in spite of change of conditions such as would, without special organiza-

[1] Chemically, HO ions.

[2] By postulating an unlimited atmosphere for "ventilation" Bayliss here obtained a greater degree of constancy than would be the case were buffer action alone operative.

tion, greatly alter conditions. In respect of alkalinity or acidity the system is, within limits, relatively self-preserving.

Many other examples of buffer solutions could be given.[1] Thus a solution containing proper proportions of the two phosphates of sodium, the mono- and the di-sodium phosphates, " will vary very little from neutrality even with considerable excess of the acid or alkaline component." A solution containing protein will act in the same way, and so on. All such solutions illustrate the fact that special organization may give a chemical system the special property of, so to speak, automatically opposing compensatory changes to a threatened change of alkalinity or acidity.

Thus buffer solutions and other reversible chemical reactions, with such mechanical systems as the pendulum, illustrate the presence in inorganic nature of complexes with what may be called the tendency towards conservation, such tendency being given in each case by special organization and acting of course in accordance with the principles of physical science. The compensation may be only partially " successful ". This is true both of the Le Chatelier and of the buffer systems, though in the latter the curve describing what happens is remarkably flat in the middle. In each of these cases special organization brings about the persistence of a pattern into which different individual elements may enter.

It is important to realize the difference between such complexes and the closed or free system. Each is a complex of elements connected by special interrelationships. Each is the product of a certain simplification and abstraction. Each exhibits a certain constancy. But the constancy is in reality a very different thing in the two cases. The one is constant because the conditions have by abstraction been made such that there is no reason why it should change. Every perturbatory agent in the mechanically free system, every source of increased or diminished energy and matter in the closed system has been excluded. If observation shows that a given complex cannot pass this test, then the complex under consideration is either extended until a free or closed system is attained, or the data are conceptually simplified, as when we imagine what would take place were there a fictitiously impervious envelope round the aggregate in question. Thus the free or closed system is constant because, under the

[1] See chapter VI of Clark, 1925.

postulates of physical science, there is, *ex hypothesi*, no reason why it should not be constant. Under the same postulates, however, there is at first sight every reason why such a complex as a buffer solution should change. In certain respects it does in fact change. The dilution alters. The number of molecules in it alters. Since these particular alterations depend on the amount of liquid introduced from outside, no underlying constant principle can be found for them within the partial complex, as such. From the point of view of the complex in question they may be said to depend on external caprice, to be contingent. And yet special organization brings about in the complex a certain constancy of its own. Such a complex provides in fact an excellent example of a partial system with its own organization for constancy.

Two distinct systemic aspects must be considered. As part of a more comprehensive system such a complex is a partial system, and when considered in certain aspects *per se*, apart from its relations to the rest of the system, it is variable or contingent; though its relationship with the rest of the system gives it continued identity as a system.[1] On the other hand, as possessed of its own internal organization for a constant pattern it exhibits its own inherent constancy. The difference between such constancy and that of the closed system is the difference between the hypothetical body of Newton's law and an automobile. Under the action of no forces the former continues at rest or in uniform rectilinear motion, while the automobile, in spite of definite frictional forces tending to bring it to rest, actually does continue to move, owing to its special organization for the transformation of energy. Such organizations as the buffer system which tend to be constant but which, at the same time, are related to a capricious environment are of great importance in the theory of the biological system. In fact the buffer systems foreshadow at a distance the biological systems, which will later be seen, all of them, to be essentially partial systems, exhibiting a remarkable constancy of process-pattern which allows for interchange of material elements with the environment. Of course it is not surprising to find in the buffer solutions such a foreshadowing of characteristically biological systemic form;

[1] Much as the sand in one half of an egg boiler may be considered logically as the same partial system while the sand is running. It is "the sand in the lower half", not just a changing heap of sand.

for the blood of the air-breathing animals actually exhibits a most elaborate and surprising variety of buffer reactions, and in fact the carbon dioxide system cited is given by Bayliss as analogous to what goes on in the blood of such animals. The constancy of the hydrogen ion concentration of the blood is one of what Cannon (1929) calls the Homeostatic or constant organic factors. Such factors are of very great importance in the living organism.

The fact that organic constancy has its elementary inorganic counterpart in such systems as the buffer solution raises a point which it is well to discuss in this place. Le Chatelier's theory is, in its strict form, deducible from the laws, which are of course really postulates, of thermodynamics. Such deduction implies nothing to the mechanism by which the compensatory shift of equilibrium is brought about. The point is well made by Edgar (1924). " Since the energy changes accompanying a reversible process are determined in general by the initial and final states of the system, the deductions of thermodynamics have the advantage that they are independent of the mechanism by which the change is brought about. This of course involves the drawback that proof of the mechanism of the change cannot be derived from the final results by thermodynamic reasoning. In applying the principles of thermodynamics to a chemical reaction, therefore, we are concerned with the relations which exist between the proportions of the reacting substances *at the equilibrium condition* and not with the mechanism by which the condition of equilibrium was attained." The same point has been made by many other writers on thermodynamical problems. It is of great importance for the understanding of the relation of organic functions to the postulates of inorganic science. Three points are really involved. Special organization may be said to superimpose special methods of functioning upon the thermodynamical laws. Secondly, these special phenomena of the specially organized system are quite consistent with the thermodynamical postulates, and may indeed be deduced from them. Thirdly, it has been found possible, by choosing suitable postulates, to study the properties of a thermodynamical system without full knowledge of all the constituent elements and relations of the system. Of the metaphysical principles involved I am not certain. But there seems no doubt of the facts. When the organism is being considered

as a system it will be well to bear in mind that the apparent uniqueness of many organic phenomena by contrast with those of the inorganic world does not necessarily mean that the postulates of physical science are being contravened ; but that it is at least a tenable hypothesis that the special organization of such living systems may, in accordance with these postulates, produce special results. In fact this is apparently the hypothesis tacitly adopted by most modern biological research. To return to the inorganic example : the buffer system operates in accordance with the laws of thermodynamics ; this means that the laws of thermodynamics operate under special conditions of organization to produce the buffer effect, and that the particular method by which this effect is produced depends upon the specific type of chemical organization involved. In the same way, under certain conditions, any reversible chemical equilibrium must shift in a certain direction according to the thermodynamical postulates. This is Le Chatelier's rule. By just what chemical changes this result is effected will depend on the peculiar nature of the system in question, and the whole process will indeed not be fully understood until more is known of the intimate nature of the atom and its association into molecules.

This has been generalized by Paul Weiss (1925) into the doctrine that it is possible to study the whole properties of systems in general without consideration of the particular internal mechanism involved. In fact, this must be done if certain problems are to be solved. For the study of the chemical system the study of individual atomic relations would only cause confusion. It is only by the study of the whole complex of molecules, considered as a whole and taken in this case statistically, that certain results can be obtained. It has been pointed out that the intricate nature of the changes involved in chemical reaction is as yet hardly known, and indeed the science of chemistry was built up at a time when our ignorance in this respect was even more profound than it is today. Similarly in investigating the properties of a pendulum, the individual intermolecular forces cannot be taken into account. True it is that the pendulum properties would be impossible without the particular inter-connections and restraints that regulate the relations of individual molecules. But it is the organization of these individual connections as, and into, a whole that gives the peculiar properties of the

pendulum. This is true even though these properties are exhibited by simplification of the total data. The pendulum properties were, again, discovered quite independently of any knowledge of atomic and molecular properties, and indeed, while we are still very much in the dark as to the nature of the constituent elements of the pendulum and the mechanism of their connection, we have a very exact knowledge of the properties of the pendulum itself. This is only possible because we can treat the complexes of atoms that make it up as units with whole properties, neglecting the internal mechanism by which these whole properties are attained. The string or rod is subject to a certain "tension". For the purpose of the mechanism of the pendulum it is not necessary to enquire how the intermolecular forces bring about the tension. Concentration upon these forces would literally lose us the wood for the trees; it would barter the harmonic properties of the pendulum for atomic and sub-atomic relations.

The further question is then asked by Weiss,[1] "Is it in itself indifferent what order of magnitude we employ in our explanation of the observed conformity to law?" Clearly not. Description must be in terms of a unit that actually exhibits the phenomena being described. The unit which may be suitable for one branch of knowledge may not be suitable for another. The clock-maker, the structural engineer, and the physicist are all dealing with the same primary material, but they deal with it in different units.

Again, to split up the clock and the bridge into the same material is to lose the characteristic quality of each. It is true enough that each consists ultimately of the "matter" with which the physicist deals, but the organization of the matter, what Aristotle would call the "form", has in each case given properties which must enter into a real description of the facts. Analysis must not, then, be pushed so far that it runs past the unit exhibiting the phenomena under investigation. This point is, of course, made by the Gestalt school of psychology and it forms the basis of much of their criticism of traditional psychological concepts.

In insisting upon the logical necessity of distinguishing the properties of the system as a whole from the mechanism by which these properties are brought about, Weiss makes the

[1] In a slightly different context but with the same ultimate ideas in mind the point is raised by Cohen-Kysper (1914).

complementary point that similarity of whole properties does not necessarily imply similarity of mechanism. " A drilling machine is not a drilling machine because this or that mechanism is running on an iron frame. . . . Someone else may solve the same problem by another mechanism but both are constructing drilling machines." In particular it may be pointed out that what has been called the property of conservation may be attained by the use of many mechanisms. In fact, Weiss makes this property the distinguishing characteristic of the system, which he defined " a complex which shows the tendency, upon alterations to its parts, to remain constant as a whole towards the external world." With a slight change of wording this definition may be brought into line with the one here adopted. The latter has, however, been preferred in this book, as seeming to arise more immediately from the usage of logic and physical science, while Weiss's statement has a more immediate biological application.

It may now be seen that the existence of complexes maintaining a fixed pattern has a certain relation to what is known in biology as the principle of the survival of the fittest (Lotka, 1925, chapter on Le Chatelier's Rule). In general the fact that a complex is so organized that it will automatically compensate for disturbances of external origin makes for the persistence of such a complex. Thus a stone " survives ", while an equivalent number of gaseous molecules dissipates. The same thing is true of the patterns ; relatively speaking, those patterns survive which are associated with complexes tending to maintain them. The greater the disturbance for which compensation can be made, the more robust may be said to be the complex or pattern. It is assumed that in a world considered as a *fortuitus concursus atomorum* all other factors are equated in the two cases. In the same way, the biological principle of the survival of the fittest states that those organisms tend to survive which have the traits making for survival value. In a sense both statements are tautologous. We say that organism or that pattern will survive which has survival value, and if the question be asked what is survival value, the only answer is that it is a characteristic making for survival. In neither case are we able to state directly *from the principle itself* which special organization will be followed by the conservative result in question. And yet, as Lotka points out, the principle of natural selection has been

of the highest value in biology in spite of its inability to predict the specific conditions that will have survival value. In fact, such prophecy would be as impossible as unnecessary. Just as there are many mechanisms by which the conservation effect may be produced in a non-living system, so there is no reason to limit the number of organic traits which may have survival value, nor to claim that such survival value must always be produced by the same means. Presumably the insects and the human race have each survived in such numbers by entirely different specific means.

Displacements of Disturbed Systems.—The discussion has led a considerable distance from the system as originally defined, which persists because both elements and relations remain identical. There are, however, certain problems which can only be understood in reference to such systems. Consider such a complex which has been disturbed by the addition of energy or material substance. The disturbance has resulted in a change from the original constant state, and it has been seen that from a certain point of view it is possible to say that a fresh system is really created. Now the postulates of physical science require that after the disturbance a fresh constant state will sooner or later be reached if the new complex is kept in isolation. That is to say, the resulting series of systemic displacements must be such that equilibrium is again ultimately established. Otherwise will there come into being some kind of " perpetual motion ".

It has been seen that such ideal conditions are never actually experienced. Nevertheless, the return to equilibrium is exhibited in many systems of everyday experience. A swinging pendulum goes on for ever in an eternal dynamical equilibrium, if it is isolated and if there is no friction. If there is friction, and friction there always is, it comes to rest, provided that no energy is transmitted to it after the first impulse. In the same way all other mechanical systems actually do come to a position of equilibrium unless energy is perpetually supplied to them. In other systems similar agencies produce the same result.

Consider such a complex as a pendulum after a disturbance. Movement takes place at any moment because the forces acting on the pendulum are not balanced; the pendulum continues to move until these forces are evenly balanced, that is to say, until there is no reason why movement should take

place in one direction rather than another. The state of rest is one where "naught occurs, for nothing can occur." [1] Every state of displacement is nearer to this position than the last. The final state is then a limiting position, to which preceding states may be thought of as approximating. In mathematical analysis such a limiting position is described, for convenience only (Petzoldt, 1890) by saying that here certain mathematical functions have a maximum or minimum value. "So much and so much only occurs as in virtue of the forces and circumstances involved can occur." And again, "Often the phenomena of nature exhibit maximal or minimal properties because when these . . . have been established the causes of all further alteration are removed" (Mach, 1893, p. 460). If now the system exhibits what has been called the property of constancy, the disturbance gives rise to a series of transitional states which tend towards the re-establishment of the state that is conserved. In the case of the pendulum this is the vertical position; with the buffer solution it is the constant state of acidity, and so on. So that the position of equilibrium is really a special or limiting case, the equilibrium being capable of being expressed mathematically in maximal or minimal terms.[2] These are the stationary and quasi-stationary states which Köhler and the other Gestalt psychologists treat. Chemical and physical science, of course, often find it convenient to consider systems in equilibrium because these are obviously of more frequent occurrence, every system coming, under the constant conditions postulated in such science, into a position of equilibrium.

It will be noticed that in the above discussion a new principle has emerged, namely that when the series of displacements is initiated the final state is, in a certain way, implied in all the preceding states. Mathematically it is said that the preceding states are successive approximations to the final or limiting state, which, as we have seen, is like all the others, predetermined. Thus Lachelier speaks of "the unity of the system which causes several movements to converge

[1] "Nichts geschieht, weil nichts geschehen kann," quoted by Weiss.

[2] It will be realized that in a partial, that is, an open system, where the surrounding conditions are indeterminate, the maximal or minimal expression does not immediately apply. It has been postulated that, after the disturbance, surrounding conditions are constant, at least for a specified time.

towards a common end " (1924, p. 74). If now the system is such that, after various disturbances of different degrees, the original position of equilibrium is fully restored, then, whatever the series of displacements, the same final result is reached. In the case of the pendulum,[1] for example, however the bob is struck within limits dictated by the intra-systemic forces—here the " strength " of the string—the same vertical position is finally reached. We can, then, say that the final position is in a certain sense independent of the details of the preceding transitional positions; when we see the pendulum moving we know that motion will continue until the vertical state of rest is attained, no matter what the intervening movements may be. This consideration will later be seen to have important consequences.

An equilibrium such that it tends, when disturbed, to be re-established is called " stable " by contrast with the so-called meta- or un-stable equilibria. These latter may be so organized that the net chemical or other changes taking place when equilibrium is disturbed have other than what we have described as a conservative direction. Thus (Benedicks, 1922) changes may be initiated which, instead of tending to bring the equilibrium back to the original position, would result in transformations progressively farther removed from the original state, until equilibrium was indeed reached, but one which was far from conserving the original factor which had been disturbed. A similar state of affairs may be seen in cases of unstable mechanical equilibrium. A cube may be balanced for a moment on one of its points, but the slightest disturbance will be followed by changes which progressively remove it from the original position, until finally a fresh equilibrium is reached, which is generally stable. A chemical example would be combustion.

The System and the Gestalt.—Thus it is possible to consider the system under either of two headings. We may discuss its properties when it is in equilibrium and undisturbed by any change of the factors conditioning such equilibrium, or we may discuss it during the transitional states between one position of equilibrium and another. The first set of problems

[1] Benedicks makes the Le Chatelier principle the criterion of a stable equilibrium (1922), thus making Le Chatelier's rule of wider applicability than its propounder originally had in mind. See Humphrey, 1930.

has been treated by W. Köhler [1] (1920). A detailed account of Köhler's analysis is here unnecessary. It may, however, be said that he apparently succeeds in demonstrating the existence of physical complexes with the following properties: They exhibit whole properties, as contrasted with properties of parts, elements, "moments". They are "more than" the sum of their parts, and are "non-additive". They are transferable, "transposable". Their whole properties are independent of the particular matter composing them and the particular quantities involved, as long as the necessary relations still exist. Köhler takes the example of a charge of statical electricity on a spherical conductor. This charge will distribute itself *as a whole* over the total surface of the conductor. The distribution *of the whole* charge will be affected by the addition or subtraction of electricity at any point. Thus, according to Köhler the charge cannot be considered as the linear or arithmetical sum of geometrical distributions of statical electricity, but must be regarded as a unit in itself with properties of its own which is "more than" the sum of the parts, and is "non-additive". Further, the spherical distribution is transferable; it can exist, for example, upon spheres of different sizes. In the same way, the peculiar distribution which is appropriate to an ellipsoid conductor may be reproduced in any scale, provided only that the correct proportions be preserved. Further, separation of a certain quantity of electricity brings alteration of the whole distribution, and so on. Another example given by Köhler is that of the drum head, the tension of which is likewise determined by the whole conditions and which may easily be seen to conform to the same canons.

Now it is above apparent that certain of the properties discovered by Köhler in the physical Gestalt belong also in an elementary way to the system in general. The system is clearly "more than" its parts. Let us return to Butler's definition, who, it will be remembered, tells us that the system is "a one or whole made up of several parts . . . but yet . . . the several parts even considered as a whole do not complete the idea unless in the notion of a whole you include the relations and respects which those parts have to each other." The system is the parts in specific relations. The same point

[1] As Köhler considered mainly the statical properties of systems, the question of stability was immaterial to his main argument.

is implied by the dynamical definition. The dynamical system is a number of particles subject to inter-connections and restraints, that is to say, it is not a set of particles alone. Thus the system is non-additive. It cannot be formed by the linear addition of discrete parts; when the parts are put together to form the system something more than arithmetic takes place, for the parts are put into functional relation with each other (Hertz, *op. cit.*, p. 418). Thus the physical system considered in its most general aspect as a complex of inter-related elements satisfied Köhler's criteria, and would probably be considered by this investigator as a "weak Gestalt".[1]

Should it then be said, as certain authors have said, that the physical Gestalt and the physical system are synonymous? (Oeser, 1930; Jaensch and Grünhut, 1929). At least it does not seem legitimate to conclude that Köhler's treatment of the physical Gestalt is therefore of no value, and that his discussion has succeeded merely in calling attention to certain platitudes of physical science. Such an opinion misses the real contribution of the theory of the physical Gestalt, which is to have applied directly to scientific thought an important philosophical distinction. This may be seen by considering a further criterion which Köhler has added to the two given by von Ehrenfels. A collection of entities, he says, is truly "summative" only when the entities are unaltered by being brought together. Thus the elements forming the Gestalt are actually altered by their association in the Gestalt. The Gestalt is then not an Und-Verbindung, an enumerative combination, but a whole of elements which are what they are because they are included in this particular complex. Each element is, so to speak, determined by its place in the whole, and the removal of any element consequently changes both it and the other members of the complex. One is reminded of Aristotle's statement in the *Poetics*, "That is no member of the whole which could be detached without anyone being the wiser."

Now this point touches on a problem that has engaged the attention of philosophers for a generation, namely, that of the

[1] Köhler makes certain restrictions concerning the transposability of the Gestalt. See Köhler, 1920, section 20. The two criteria were of course taken from von Ehrenfels, who again says that he took the hint from Mach. See Stebbing, 1930, pp. 203 ff. for a discussion of systems having a similar structure.

internality or externality of relations. The point at issue is whether, by entering into a relation, a thing is modified in its essential, internal nature, or whether the relation may be entirely external to what is related. In the one case, the thing is fundamentally unaltered by becoming a term in a fresh relation; in the other to enter into a relation must bring a fundamental change to the entities involved.

Now this is not the place for a discussion of this difficult problem, neither is it necessary for our purpose to take one side or another. It is only necessary to say that many philosophers of the highest repute hold that all relations are necessarily internal, and that many hold that some are internal, some external, while others again, perhaps in a minority, hold that all are external. But it is now clear that Köhler has applied to the conception of the mathematico-physical system the doctrine of the internal relation, holding as he does that there are at least certain physical systems the elements of which are fundamentally modified by their membership in the system, that is to say, by the fact that they have been brought into relationship with the other members of the complex. If then one holds that relations are necessarily internal, all physical systems must come under Köhler's rubric and the physical system is then synonymous with the Gestalt. If one holds that some relations are external, others internal, it is possible to consider that certain physical systems come under Köhler's heading, namely, those in which the relation between the elements are internal. One who refuses to admit the possibility of internal relations must reject the physical Gestalt altogether. In any case, Köhler has performed a service of a very high order by this direct application of philosophical concepts to scientific thought.

This concludes our general account of the concept of system. It may here be pointed out that the discussion has been confined mainly to examples taken from the physical sciences, not because these sciences furnish the only legitimate use of the term, but because they do furnish the most highly elaborated instance. That is to say, the system as dealt with by physical scientists illustrates the application of the concept of system under the special restrictions and conditions which have been found expedient for the description of non-living matter and its properties. How far the properties of the system may be generalized it is perhaps impossible to say at

the present stage of knowledge.[1] At any rate, no thoroughgoing attempt at such complete generalization is attempted in this volume.

Summing up the discussion it may be said that from the definition of a system as a complex of elements connected by special relations there follows the concept of such a system as logically separable from the outside world, though the two are in experience inseparable. Such an aggregate will stand to the individual elements in the whole-part relation, though the whole is not to be thought of as merely the sum of the parts. Material aggregates exhibiting the above properties are treated by physical science under the name of systems. The fundamental assumptions of physical science imply that when such an isolated complex has been disturbed there will ultimately be a return to a constant state unless work has meanwhile been done on the system from an outside source. When this occurs certain mathematical functions have a limiting value. A system may be so organized that such disturbance is followed by displacements tending to reinstate some special feature of the original state. This has been termed the property of conservation.

The living organism will next be considered as an example of such a complex, exhibiting the same fundamental, whole-part property, and there will be noted various similarities and dissimilarities to the types of complex already considered. This will form the general topic of the next chapter.

Note.—Other definitions have been given of the term system. Eisler's *Wörterbuch der Philosophischen Begriffe*, Art. "System", has "ein ganzheitlicher Zusammenhang von Dinger, Vorgängen, Teilen, wobei die Bedeutung jedes Teiles vom über-geordneten, über-summativen Ganzen her bestimmt ist." This may be seen to accord with the ideas of the "Gestalt" school; the tender points in the definition are perhaps the words "Bedeutung" and "über-geordneten". Again, according to Paul Weiss, "als System wollen wir mithin jeden Komplex definieren, welcher das Bestreben zeigt, bei Änderungen an seinen Teilen, als Ganzes nach aussen hin konstant zu bleiben" (1925). That is, the "survival power" of the complex is made its distinguishing mark. Of course, every complex must have survival power to a certain degree or it would instantaneously and totally change, and thus not be treatable as a complex. I feel, however, that the whole-part property is perhaps more fundamental, and that the "survival" property may better be treated separately. Avenarius, on the other hand, makes the

[1] Such a generalization has been attempted by Weiss (1925).

distinguishing mark of the system dependence of alteration in one element upon alteration in another element (1907). This has ultimately the same implications as the dynamical definition. In fact Hertz (*op. cit.*) first defines a system as a number of material points considered simultaneously (p. 46), and later specifically adds the notion of connection. A connection exists when, from a knowledge of some of the components of the displacements of those points, we are able to state something as to the remaining components (p. 78). This defines the dynamical systems, which are actually treated, in terms of dependence of alteration of one part on that of another. Bosanquet accepts Avenarius' description in one passage. Lalande's Dictionary defines a system as a system (H. R. MacCallum). He calls it " The ensemble of material elements which reciprocally depend on each other in such a way as to form an organized whole " (Lalande (1928), Art. " Système "). Miss Stebbing incidentally defines a system as a complex of interrelated elements. " A set of related things which may be regarded as a system " (1930).

CHAPTER III

THE VITAL SYSTEM

"To natural effects of the same kind we must as far as possible assign the same natural causes, *e.g.* to respiration in plants and animals, to the descent of stones in Europe and America, to the light of our kitchen fire and of the sun, to the reflection of light on the earth and on the planets." (Newton, *Regulae Philosophandi.*)

"Whatever future investigation may teach us concerning the particular physical and chemical conditions concerned in the development of the organism, its similarity in many respects to non-living dynamic systems is, I believe, highly significant." (Child.)

THE use of the term system to describe living creatures is old and widespread. Apparently the first instance is to be found in the Greek rhetorician Dionysius of Halicarnassus, who speaks, about 30 B.C., of "the whole system of the body" (Liddell and Scott, 1873). In 1729 Bishop Butler (Murray, 1920) uses the phrase "the body is a system". In the early nineteenth century the term was employed by Cuvier (1831) and also by the French anatomist, Vicq d'Azyr (1805). Classical examples of a somewhat later date are the works of Lotze (1885) and Avenarius (1907), each of which writers makes extensive use of the concept of the inter-connected system to describe organic phenomena. Among more modern writers are Lotka, who refers continually to "living systems", Rignano (1923), who states that "every organism is a physiological system in a stationary condition," and Bayliss (1923), who tells us that "the cells of these beings" (*i.e.* living organisms) "are heterogeneous systems." The same author refers to the amœba as a system. Verworn speaks of "vital systems" (1913), Pavlov (1927), of a "definitely circumscribed, material system." Child uses a similar terminology (1924, *passim*), Cohen-Kysper devoted a book to the consideration of the organism as a living system. The later examples are taken almost at random from the great mass of literature in which the term has been used. Its implication is, of course, often vague, and this is more especially true of the earlier

instances; but the term is today used by authors of too high a standing for it to be dismissed as a pious metaphor or as one surreptitiously introduced in order to bring the flavour of exactitude to a subject hitherto regarded as essentially incapable of precise treatment. On the contrary, it will appear in the sequel that, when used with discretion, the concept is one of real descriptive value both for biological and for psychological thought.

Bearing in mind then the discussion of the last chapter, *the organism may be said to behave as an intricate system of material processes, tending actively to maintain a complex pattern under constantly changing conditions. The system is part of a more inclusive one comprising both the organism and its "environment", and is thus a partial system with a specific organization of its own. The constitutive relations of the system may be described as biological, without, however, implying that there are involved processes unique to the organic world. In what are known as the higher forms* [1] *of life this maintenance of pattern is effected by means of a complex hierarchy of partial or sub-systems, which may or may not be themselves directly related to the environment. The biological system differs from the majority of those hitherto considered in that it may be composed of different material particles at different times, the total pattern, however, being independent of such variation;* of such maintenance of independent pattern we have, however, seen certain primitive examples in the inorganic world. *The organism is at any moment the patterned parts, and because the pattern continuously exists, though it may be modified, we speak of the same organism and the same biological system at different times.* When in this book and in the authors cited the language used necessarily implies that we have to do with the same animal throughout a lifetime, the reservation is implicit that the sameness is one of patterned, not of identical constituents. It is the same organism today and tomorrow, just as "Father Tiber" was the same river before and after Horatius took his famous plunge.[2] Indeed, one of the most striking characteristics of

[1] And to a certain extent in all forms.

[2] This logical procedure here involved is of course not confined to the organic sphere. If we do not adopt it, Kratylus pointed out that we must say not only that we do not go into the same river twice, but that we do not go into the same river once. Unless we allow such identity with change of material substance we cannot speak of a river at all.

the organism, considered, as a system, lies precisely in this, that, although it is essentially partial to the environment, with which it continuously interchanges matter and energy, yet it does tend at any one moment in its existence and under "normal" conditions to maintain an identity which is more than the logical identity of the partial system, and which is again not static and unchanging but *dynamic and progressive*. In this respect it may be compared with the chemical systems obeying the Le Chatelier rule, and the buffer solutions considered in the last chapter, which shift their equilibrium while at the same time tending to maintain conditions as they were before the change. Like any other partial system the organism alters with alterations in surrounding conditions. Its distinguishing feature is found in the fact that in it stability is approximated by a highly efficient mechanism, which is itself subject to development and modification.

Before dealing in detail with the questions raised by these statements, there may be repeated a caution which has already been given. In thus describing the organism as a system with a highly intricate maintenance-property no attempt at final description or explanation is being made. The particles composing the living system are so organized that they tend towards a certain conservation. It is not claimed that such a statement, even though it were elaborated, would fully exhaust the properties of the organism. Such a supposition would be wholly misleading. We do not presume to final description any more than does the chemist when he calls a certain aggregate of material particles calcium carbonate, or the geometer when he says that the bees' cell is hexagonal. Like the description of these and all other scientists, our account will involve simplification and a certain amount of abstraction; like theirs it will, nevertheless, we may hope, prove serviceable.

The Organism as a Unity.—It is scarcely necessary to point out, to begin with, that the organism and its component members stand to each other in the whole-part relation. These members are themselves organized out of the ultimate chemical substances of which the living creature is composed; but by the parts of the organism we usually mean the organized members, although strictly speaking any constituent molecule, of course, forms part of the total system. It has been seen that such interrelation of parts constitutes a unity. "It

is", says Claude Bernard, "the subordination of the parts to the whole which makes of the complex creature a connected system, a whole, an individual," [1] or in the words of a later authority, "The living creature is fundamentally a unity. . . . It is as a whole, a single unity, that the animal, as for that matter the plant, has finally and essentially to be envisaged." [2] Perhaps nowhere indeed is the relation of part to whole better realized than in a living organism—living, for at death the essential unity is destroyed. As Aristotle picturesquely remarked, a dead finger is a finger only in name. The relation of the living creature to its bodily structures is the whole-part relation *par excellence*. This wholeness of the organism, its "unity", has been recognized by the coinage of the term "individual", which is used to designate a single organism, but which etymologically implies indivisibility. Physiologically, the point has been elaborated by Sherrington (1911) who exhibits the neural mechanism by which the unity is achieved, and again by writers of the "organismic" school, such as Ritter, in his book, *The Unity of the Organism* (1919). The wholeness is possible because the organism is structured, so that it consists of inter-connected elements, the maintenance of the integrity of higher organisms being the elementary function of the nervous system (Coghill, 1929, p. 90).

Organic wholeness is excellently illustrated by certain embryological studies that have lately been made. Coghill, for example, has made a detailed investigation of the embryonic water-lizard with the object of correlating the development of its behaviour with that of its nervous system. He found (p. 36) that "the behaviour pattern", as was perhaps to be expected, "develops in a regular order . . . which is consistent with the order of development of the nervous system and its parts", and that "Behaviour develops from the beginning through the progressive expansion of a perfectly integrated total pattern and the individuation within it of partial patterns, which acquire various degrees of discreteness." There is, according to Coghill, even perfect integration before the animal has a nervous system at all. This effect is, he says, accomplished by means of physiological gradients, or axes of gradually increasing physiological activity within the organism. These, he claims, it is reasonable to regard as "expressions

[1] 1878, p. 363. [2] Sir Charles Sherrington (1922).

of the integrating forces of the organism as a whole." Similar gradients, he thinks, determine also the track which the growing nerve cell follows, the new structure thus determined interrelating still more intimately the elements of the system as a whole. Thus a relatively primitive unity determines, by processes which are the expression of its "oneness", development in the direction of a still higher unification. The whole growth of the organism follows the same rule: the part is a part in relation to the whole, so that in the growing creature at every stage we have not an arithmetical sum of anatomical members but an interrelated unity. Coghill further calls attention to the work of Magnus (1925) as stressing the solidarity of the whole reflex pattern by which the individual reflex is subordinate to the total unity of behaviour. Thus from its very inception as a fertilized ovum and throughout its development as an embryo this elementary organism exhibits the wholeness property characteristic of all living creatures.[1]

Closely connected with this "unity" or "wholeness", and equally characteristic, is the organism's separateness from the outside world. It has an "inside" and an "outside", the "outside" consisting, at any time, of all material particles other than those in the special unifying relations of the (partial) living system in question. The distinction is recognized in popular thought and in the phrase "organism and environment" which has already been used—environment *being* the "outside" of an organism, what has been called philosophically the "non-I". It should here be remarked that, because it is exceedingly hard at times to draw the dividing line between the organism and the environment, that is to say, between the partial and the rest of the total system, it does not follow, as is sometimes assumed, that the distinction therefore does not exist. The relation is such that the two terms are in practice inseparable, for it is impossible to have an organism without an environment; logically, however, or better ideally, they are separable. Examples of a similar relationship are to be found in the length and breadth

[1] For a discussion of various theories which stress the organism-as-a-whole, see Bertalanffy, 1928, Ch. XI. It is not of course here denied that there may exist in such a complex organism as the human being, *relatively* independent patterns or "complexes", such as, for example, Lewin postulates. See Lewin, 1931.

of an area, and in Aristotle's excellent instance of the concave and convex side of a curved line.

It has been pointed out that the separateness of the organism and the environment is achieved in spite of continual interchange of energy and material with the outside world, because what persists as the organism is, more or less and within limits, independent of the particularity of the matter involved.

Certain writers have ascribed special properties to the unity of the organism. Thus it is often said to be "prior" to its parts. Kant, for example, claims that "The cause of the particular mode of the existence of each (organic) part resides in the whole, while in non-living masses each part contains this cause within itself." Similar language is used by Aristotle. Now it is dangerous to concern oneself with the doctrine of final causes, which in the past has often been detrimental to scientific progress. However, with the proper reservations such statements as the above do express a real fact, which, nevertheless, does not appear to be confined to the organic world. It is legitimate to say that the organism is "prior" to its parts inasmuch as the parts are determined by the total pattern.[1] Enough instances have already been given to make this clear. Child's conception of axial gradients is an excellent example; one may legitimately think of the behaviour of organic matter as being determined by its place in the gradient. In the same way, Coghill's work already quoted shows a "dominant mechanism of integration of the whole organism." A similar physiological dominance of the whole may be seen in the process of assimilation of the higher organisms. Here material is taken into the body, digested, and transformed into substances which are available for the needs of the organism in general. According, however, to the specific needs of the total structure in different parts, the immediate biological fate of this available material is different. It may become part of a lung cell, a liver cell, or of a muscle. Whatever be the immediate fate of ingested material, however, ultimately it is the needs of the total organism that are served. Thus ingested carbohydrates are split up into single sugars, which are carried into the blood stream, where they are either taken directly to the tissues of the body or stored as animal starch in the liver. In either case they ultimately release

[1] This is a biological application of the philosophical doctrine of internal relations.

energy for the general purposes of the body by oxidation in the muscles. Similar transformations are undergone by the other products of digestion. Transformation of ingested matter takes place as local exigency dictates, the ultimate result being, however, perpetuation of the total pattern. While, therefore, one should beware on the whole of teleological explanation, yet there does seem a very legitimate sense in which it may be said that the whole body and its need determine what happens in the parts, and, in Kant's phrase, " the mode of existence of the parts." It may be a question of emphasis only that is involved in the difference between the teleological and so-called mechanistic descriptions.

Very similar is the statement that the body is " more than " the arithmetical sum of its parts. Like any other system, the body is its parts in relation to each other. It is not simply a juxtaposed aggregate of the ingested food, but it is the ingested food modified, as we have seen, according to the conditions of maintenance of the total pattern. The ingested material is altered when it becomes part of the organism. Thus the organism satisfies the criteria of the Gestalt school for a structure as contrasted with a summative aggregate. The relation of the systems satisfying these criteria to the system in general has been discussed in the last chapter. Here it is sufficient to point out that in general biological systems apparently come under the Gestalt rubric,[1] and, by the same token, that biological relations are as a rule what the philosophers have termed " internal ".

The Organism as a Partial System.—It has been insisted that the organism cannot be considered as a free or total system. Strictly speaking, it is part of a more inclusive structure composed of the living system together with its environment; for of its very nature it stands in perpetual and superficially varying relations to the environment. It is only by a process of abstraction and simplification that a living body can be separated from the surroundings. Indeed it is often difficult to determine where organism ends and environment begins.[2] Yet such logical separation is justified

[1] Compare Matthaei (1929, p. 79), *Organismen sind Gestalten.*

[2] Haldane (1929, p. 81 *et passim*) has insisted upon this fact. He states " we cannot, even in thought, separate the structure from the . . . environment and its influence, unless we entirely neglect what actual observation shows us to be their real characters." I take it that

because of the fact that the parts of the organism do stand to each other in a special nexus of relations in which the environment does not share. Yet the living system is strictly bound up with the environment, and in two distinct ways, which are justifiably distinguished in popular thought. The organism is essentially capable of stimulation from without. It also has what are known as "needs", such as food, drink, and air, which are necessary for its continued existence. These must periodically be taken up into the organism in order to conserve its structure and to balance the energy it continuously expends, and systemic energy is likewise expended during the process of acquiring them. To some the use of the term "needs" will seem to flavour of subjectivism and the plea of *sui generis*; but while the term has been misused in the past, and in such a way as to give a deceptive appearance of finality to certain biological and psychological phenomena, yet there seems no question that it draws attention to a very real class of biological phenomena. The two peculiarities of the living organism, its Irritability and the fact that it has nutritive and other needs, together give a sufficient reason why any treatment of the organism which considered it as a closed or isolated system would rest on an artificial abstraction, and would do violence to essential biological fact. The organism is not "isolated" from the environment; it is continually in stimulus-response relationship with it, and continually exchanges material and energy with it.

Because of this intimate organic relation it would, as Köhler

Professor Haldane here means, by "separate in thought", "treat as biologically separable", not, of course, that the organism and its environment are logically indistinguishable. Biologically, one may say, they are inseparable, logically they are separable. For when he writes "we cannot, even in thought, separate the structure from the environment," Haldane is of course himself making a logical distinction between the two terms. Clearly there must be something for the environment to influence, and which has a measure of real independence of the environment. This is the organism. Similarly, when Professor Haldane says "we cannot regard the external environment as outside of life," (p. 11), he means, I take it, outside in the sense of "not involved in the vital phenomena", rather than "spatially external to those particles of matter which stand in the particular relationship constituting the organism." Practically it is, as Professor Haldane points out, very difficult to say where the dividing line lies, just as it is practically very hard to say where the middle point of a line is.

has pointed out, be illegitimate to apply to living processes such simple dynamical considerations as are appropriate in treating the isolated physical system, even though such isolation is as a matter of fact always the product of abstraction. We cannot, for example, think of the organization of a (partial) biological system as *necessarily* giving " direction " to the phenomena occurring within the system, a result which Köhler has shown to follow for the simpler and isolated physical system [1] (1929, p. 141). For the equilibrium to which, according to the postulates of physical science the isolated material system necessarily tends may, in a partial system such as an organism, be disturbed at any moment by the action of the environment. Thus such a system does not necessarily come to a state of equilibrium. Köhler takes the case of a drop of oil in liquid of the same specific gravity. Under the influence of the total molecular forces such a drop changes its shape until the spherical form is reached, when the area in contact with the water is minimal. Thus the totality of molecular forces gives direction to the changes in the drop. The drop is, of course, a partial system, and it is assumed that conditions in the rest of the system, that is to say, in the surrounding fluid, are constant. Without this specific and artificial assumption it is not possible to say that the outcome of the changes in the drop of oil will be a spherical shape, and in fact, except under elaborate artificial conditions, such shape will not necessarily be reached even in approximation. Biological systems, then, being partial systems, do not necessarily achieve a constant state, and thus do not necessarily give direction to the processes occurring within them. For any possible constant state may at any time be disturbed

[1] " We have ", he says of certain simple isolated dynamical synamical systems, " one resulting force at each point at each instant of time. All the resultant forces together form one texture of stresses. From the principles of physics one can deduce, therefore, that *for the system as a whole* the immediate effect of all those forces will have one definite direction. At each point, the forces will produce changes of movement or process which, when considered in their totality, bring the system nearer to the balance of the forces themselves" (1929, p. 141). " If we consider systems in which inertial effects are eliminated, this principle is perfectly illustrated. Here the effect of the restraints caused by the organization of the system is at any moment in the *direction* of equilibrium."

either by the intake of matter form the environment, or by "stimulation".

Further, even supposing that the environmental conditions were constant, a state of affairs which is never achieved in the surroundings of any animal, there is no *a priori* reason why, as a system with its own stores of energy, the organism should come to a state of equilibrium consistent with the peculiar relations that make it a biological system. Without special provision, there is no reason why any hare, once started, should not run till it dies. This point belongs to a later stage of the discussion, when it will be made clearer.

Even less justifiable would be the assumption that because the organism is a system, the events which take place within the system must therefore tend in the direction of that preservation of the organic pattern, which alone can bring the persistence of any form of life. From the fact that the organism is a system it is not possible to assert that equilibrium must be re-established in a living creature; nor, specifically, is it possible to deduce a general direction for organic processes, nor that particular kind of Direction which alone can be of biological service.

The Tendency towards Self-Preservation.—And yet under ordinary circumstances the biological system actually does give "direction" to the processes occurring within it. Indeed, one of its most remarkable characteristics, and one by which we sharply distinguish it from most inorganic aggregates, is the fact that under ordinary circumstances organic processes do actively tend toward the conservation of the whole patterned complex. "Biology must take as its fundamental working hypothesis the assumption that the organic identity of a living organism actively maintains itself in the midst of changing external circumstances" (Haldane, 1922, p. 391).[1] From the lumbering resident of the prehistoric marsh to the most graceful creature of the air; from the chick making its first peck to the old man struggling for life on his death-bed, all living matter shows this tendency to self-preservation—*se in suo esse conservare.* So fundamental is this notion to our

[1] In a famous passage Frédericq has enunciated a similar empirical proposition. "L'être vivant est agencé de telle manière que chaque influence perturbatrice provoque d'elle même la mise en activité de l'appareil compensateur qui doit neutraliser et reparer le dommage" (1885, p. XXXV).

popular and scientific conception of life that students of the biological sciences must be warned against using "teleological" language, language, that is, implying that a part of the body has this or that "purpose" in furthering the conservation of the whole organism; for this is a metaphor that is dangerous in the hands of a novice, though it may perhaps legitimately be used by one who recognizes its implications.[1] Such language cannot be admitted when it carries any assumption as to the process by which, in the course of evolution, the organism acquired this organization, nor specifically when it implies that the mechanism was arranged by some foreseeing intelligence. When used to exhibit the fact that the body is so organized that processes occurring within it actually do tend in general and in many surprising details to the conservation of the whole, it is a vivid and almost indispensable method of description. Such organization of intra-organic process for organic conservation has been recognized in a more extended form by moralists of many centuries under such terms as "selfishness", and from Thrasymachus to Le Dantec ethical doctrines have been built upon the same principle. The law of self-preservation is said to be the first law of nature; it is so physiologically as well as behaviouristically. We are not concerned with the details of how the organism effects this Direction of its actions towards the conservation of the organic pattern; a full knowledge of such details would imply a far greater progress in biology than we have at present reached. Rather must the principle be accepted as a postulate, the intimate mechanism of which will, it is hoped, be gradually laid bare as biological research progresses.

It has been seen that this Direction towards preservation of the total complex, which must be postulated for all living substances, is not deducible from the fact that the organism is a system. Special organization must therefore be postulated to effect it. All living systems must then be considered to have such special organization that, in changing environmental and even systemic conditions, compensatory processes occur leading towards an equilibrium or constant state which is conservative of the system. If any given complex is endowed

[1] As when Martin speaks of the eliminative tissues, "whose main business it is to get rid of the waste products of the various parts of the body" (1917, p. 31).

with such organization, it will in general survive. Such organization has then biological survival value, this, again, being emphatically not deducible from the fact that the organism is a system. It may be pointed out that an aggregate of material particles forming a living system will survive for a certain length of time as a material configuration after death has ensued. Such constancy is, however, only a small part of biological survival. In the same way it was seen that stability in a chemical or other inorganic system is not deducible from the fact that the aggregate in question is a system, nor from the same fact can we tell whether the chemical system follows Le Chatelier's rule,[1] nor whether it will function as a buffer solution. In general, such biological systems as are to be observed actually do exhibit the conservation tendency to a very high degree, and thus are possessed of organizations with high biological survival value. Ours is, after all, an age-old world. It contains not the brand-new products of a creation-day but the battle-scarred veterans of many thousands of years of evolutionary war against environment and against other living beings. It may then be expected to contain predominantly those species which are internally so constituted that the direction of their organic events tends to the conservation of the individual living system. Such conservative direction will then be postulated in what follows. We cannot, we have seen, explain in every case its mechanism; that is the province of physiology. Nor can we explain how it originated, for that is the province of biology. It appears, however, to be found wherever life is found: "We can characterize protoplasm as a regulated chemical system of such a kind that disturbance of the normal structure or composition at once determines constructive and reparative processes that tend to restore the normal condition" (Lillie, 1920). With Haldane, we shall take the property of biological conservation as a fundamental hypothesis, recognizing that the postulate cannot be deduced from the fact that the organism is a system. We shall assume, that is to say, that the organism is a system of a specific kind, the interrelations of which are such that, in the presence of factors that would

[1] See Lotka (1925) for an illuminating chapter on Le Chatelier's law, and its relation to the principle of the survival of the fittest. Though closely related, the conceptions of stability and of the "survival of the fittest" are not synonymous.

otherwise make for change, compensatory organic processes are initiated which tend to maintain biological identity.

The Organism as Maintaining a Pattern.—Now it has been seen that the continued identity of the organism is independent of the particularity of the matter composing its parts. What is maintained is a pattern. Aristotle might say, that organic constancy is one not of matter but of form. In more modern times, fundamentally the same thought has been expressed in chemical terms by another great scientist, Ostwald (1903). He points out that the organism should be contrasted with non-organic structures in that, while the latter are for the most part composed of substances which are chemically stable, the organism on the other hand is made up of substances in continual process of change. Yet its chemical composition remains remarkably constant, the same compounds persisting, these being, however, formed out of a fresh set of chemical particles. Thus in general the energetic conditions and chemical constitution of the organism remain approximately the same, this result being achieved by the utilization of outside sources of energy, which are transformed by the organic processes in such a way as to leave a certain amount of power at the organism's disposal. From the chemical point of view the constitution of the organism with its constancy-in-flux is described as stationary, in contrast to the stable constitution of most inorganic forms. As a parallel Ostwald adduces the example of a flame, which remains constant in spatial and chemical form, while at every moment new particles enter into its actual composition.[1] In this constancy of chemical constitution with change of actual substance we have an instance of the contrast of form and matter which would have delighted Aristotle's heart. The contrast is well seen in the instance of digestion which has already been given. In Aristotle's terminology digestion and assimilation would be said to give the form of the organic body to the matter coming into it from without, while in Kant's terms the cause of the particular mode of existence of any such piece of externally introduced matter lies in the whole, for the final fate of any particular particle of ingested matter depends upon local exigencies as determined by the total structure.

Yet organic pattern should not be thought of as constant

[1] The "whirlpool" simile has often been used to illustrate the same point, *e.g.* by Huxley and Cuvier. See Merz, vol. II, p. 406.

throughout the individual's lifetime. At no period in the life of any living creature can we say: organization and the patterned parts it produces are exactly as they were last week, the day before yesterday or the hour before last. The elementary facts of growth, maturity, and senescence show a gradual alteration of bodily organization; the pattern at any moment is an incident in a life cycle which is under normal conditions and in its general features approximately the same from individual to individual. This variability of pattern is stressed by Child. "It is", he says, "at least a pertinent question whether the condition of a living organism is ever the same after the action upon it of an external factor as it was before . . . the reactions to external factors very commonly consist in more or less permanent alteration of the organism in some way rather than in a return to the pre-existing condition." Child prefers to think of the living organism as undergoing a succession of different equilibria, each equilibrium slightly modifying the pattern. It is a valuable contribution to have pointed out that organic, like many cases of inorganic, equilibrium may constantly change its level. This change may come with relative suddenness, as in the case of transportation to high altitudes, when the alkalinity of the blood is modified, or by continued stimulation, as appears to happen in the case of ordinary development and senescence. The changing aspect of organic pattern is especially important when considering such problems of form as those examined by Child, the development of the individual being here seen as a series of modifications of pattern. It must not, however, be allowed to blind us to the fact of organic constancy. For while it is true that each change of conditions brings about a fresh organic equilibrium, yet this is a special kind of equilibrium, one that tends, under normal circumstances, towards the continued existence of the organic pattern as it was before. It is a "biological" as contrasted with a "physico-chemical" equilibrium, and is conditioned by the internal organization of the living system as well as by the character of the environment.

Now this distinction is important for the understanding of the living system. It may be illustrated by supposing a particle of matter to fall on the eye of a higher organism. Tears are ordinarily secreted and the foreign matter washed out. If this first defence is not effective, other means are

used until the particle is removed, or until a permanently damaged eye results. That is to say, organic action is initiated until equilibrium is again restored, generally by the removal of the offending particle. This equilibrium may be called biological. After re-establishment of equilibrium the individual is slightly modified, the modification being part of his "experience". The difference is small, but it exists.

Contrast with this what may happen if the particle is larger and is not stopped by the eye but enters the brain. Here the organism again comes to a condition of equilibrium, which is different from that before prevailing. We say he "dies"; and, supposing that his body were kept from outside influences, sooner or later a physico-chemical equilibrium would ensue. In the first case, that of biological equilibrium, the organization of the living system brings about an equilibrium tending towards maintenance of biological pattern; in the second case this does not happen. Even in gross cases of modification of pattern, as when a worm is severed and a new individual grows from one of the parts, the equilibrium reached after disturbance is still mediated by the bodily organization; so that something very much nearer the original pattern is achieved than would result from the attainment of a sheer physico-chemically constant state, such as is reached after the worm is crushed. That is to say, biological equilibrium is one that tends, by and large, towards conservation of the disturbed pattern. Beneath biological change lies biological conservation. When dealing with biological equilibrium this peculiarity must be borne in mind.

The fact is that these aspects of organic change and conservation are supplementary to each other. An organism that perpetually became altogether different at successive instants of time would be an absurdity, for it would never be the same organism long enough for us to call it so. An organism that never changed would be an impossibility. We must think not only of the organism, but also of its organizing pattern, in four-dimensional terms; once initiated, the pattern develops, is modified, degenerates, but until the time of its dissolution it preserves a continuity and lawfulness of development, and under normal conditions, a general identity of form given by the biological organization for constancy, which causes us to say that it is the same pattern in spite of change.

Objectively considered organic identity is then rather far

removed from the continued identity of an inert thing, or that of many of the simple systems considered in the last chapter. Not identity of matter, nor identity of form, gives it, but a spatio-temporal unity of developing and changing form; we may say that the gradually changing pattern of any organic system is a single long event, conditioned by external change, at the same time regulated from within, and taking place, as do all events in our experience, in four dimensions, three of space and one of time. It is this long event, this four-dimensional unity of pattern-process occurring in definite relation to the environmental flux, that enables us with propriety to speak of Philip drunk and Philip sober on the morrow. From such a transcendentally unified complex we find ourselves in many cases abstracting; so that we speak of the organism as if it were a complex of particles unified by relations which are, at any moment, such that form and function is in general preserved and a conservative direction given to organic happenings. This will be to combine, as a working approximation, the older conception of Regulation, according to which the organism when disturbed swings back to a normal state, with the doctrine of organic equilibrium.[1]

Fundamentally, however, the organism is a system of processes, or happenings, rather than of material particles. In contrast to Victorian science, which considered the universe ultimately to be composed of indestructible things, modern science treats the world as comprising processes or events. In the preceding chapter it was possible, by making the conventional abstraction, to illustrate certain general properties of the system on the tacit assumption that the elements of the inorganic system are three-dimensional particles not involving the time dimension. In the case of the living system such abstraction, though often convenient, may easily lead to error. It is at all times essential to remember that the organism is a *unity of interrelated events, a single process comprising interrelated processes.*

Specific Subsidiary Constants in the Higher Organisms.— While the right has been reserved to make certain postulates without specifying the necessary mechanism involved—a reservation which has been seen in the last chapter to be parallel to one made by the science of thermodynamics—yet

[1] See Child, ch. xiii.

there are certain hints given by modern physiology which will enable us to obtain a glimpse of the method by which this organic direction is maintained. *The higher living systems are so organized that in addition to the general bodily integrity they maintain at a remarkably even level a large number of subsidiary constants.* The point was first made by Claude Bernard. That great and highly original physiologist claimed that every higher organism lives in two environments, an external and an internal one, the latter being composed of the fluids of the body, and in particular the blood. The internal environment may act upon the body equally with the external one, giving a false appearance of spontaneity to the organism's behaviour. "We suppress in our explanations the invisible internal environment, in order to exclude everything from consideration but the external environment which is under our eyes. Thus we are able falsely to believe that there resides in the living being a vital force which violates the physico-chemical laws of the general cosmic environment" (Claude Bernard, *La Science Expérimentale*, 1890, p. 317). In order that the delicate chemical operations of the body may proceed, this internal environment must be kept constant. In fact, the maintenance of this constancy is, according to Bernard, the all-important business of the body. "It is the fixity of the *milieu intérieur* which is the condition of free and independent life . . . all the vital mechanisms, however varied they may be, have only one object, or of preserving constant the conditions of life in the internal environment" [1] (1878). Haldane, who has adopted this conception of Bernard's, exhibits specific instances. He reports that when large quantities of water were experimentally drunk, more urine was secreted, containing, however, less sodium chloride than usual. The blood had been maintained at the same saline concentration and volume. If the usual supply of water to the body ceases, the blood volume is increased out of the water reserves. Similarly, the circulation rate is adjusted to secure an approximately constant blood composition at any part of the body, and an approximately constant tension of carbon dioxide and oxygen in the blood. The sugar concentration is kept constant by means of the sugar reserves in the liver, and mechanisms likewise exist whereby the percentage of albuminous substance is kept constant. Despite external variation of condition or severe

[1] Professor Cannon's translation.

work on the part of the organism, temperature is kept constant within narrow limits. Even though the heat production in the body is increased six- or eight-fold, the temperature of the arterial blood scarcely rises, the relatively enormous quantity of additional heat being dissipated by sweating and increased circulation, and similar changes take place to compensate for alterations of external temperature. There is, further, a mechanism for regulating the formation and destruction of blood corpuscles (Haldane, 1917, Chapter III).

This maintenance of bodily constants is treated more exactly by Cannon (1929), and is called by him homœostasis, although he expresses himself as not entirely satisfied with the term. Homœostasis is one instance, perhaps the key instance, of the "organism's capacity of retaining its integrity in spite of ceaseless metabolism," to use Thomson's phrase (1920, p. 101). Cannon's list of homœostatic categories is as follows:

A.—Material Supplies for Cellular Needs.

1. Material serving for the exhibition of energy and for growth and repair—glucose, protein, fat.
2. Water.
3. Sodium chloride and other inorganic constituents except calcium.
4. Calcium.
5. Oxygen.
6. Internal secretions having general and continuous effects.

B.—Environmental Factors affecting Cellular Activity.

1. Osmotic pressure.
2. Temperature.
3. Hydrogen ion concentration.

As a typical example, and one which has of late attained considerable importance in biology, we may take the last of these constants, namely, the hydrogen ion concentration. The blood has a remarkable power of maintaining the alkalinity-acidity relation within the very narrow limits outside of which the component cells cannot live. The narrowness of these limits may be appreciated from the following considerations. A frog's heart dies if the fluids circulating through it change from the normal condition of approximate neutrality

to one of very slight acidity. This result may be produced by a degree of acidity so slight that it cannot be perceived at all by the delicate sense of taste and is hardly detectable by the litmus test of acidity. Such a solution may contain no more then four parts of hydrochloric acid in a hundred million parts of water. "Changes in the concentration of hydrogen ions amounting to less than one-tenth of this magnitude may produce profound, even if not fatal, effects on many living cells" (Mitchell, 1923, p. 157). In order, then, that the body may be kept in normal good health, the acidity-alkalinity equilibrium of the blood must be preserved within amazingly narrow range of variation. This result is effected by a complicated set of "buffer" reactions, of which the two chief are effected by a bicarbonate-carbon-dioxide system, as described in the last chapter, and a phosphate system. So efficient are these buffer reactions in the blood that it is said that approximately a quart of concentrated sulphuric acid may be poured slowly into a volume of blood equal to that of a man without appreciably affecting its neutrality. This exact and powerful mechanism for maintaining a bodily constant is typical of the general activity whereby constancy of organic form and function is attained.

Now the buffer solution of the laboratory does not preserve, even in theory, an absolutely constant acidity or alkalinity. It rather diminishes to a remarkable degree the change produced by the introduction from without of acid or alkali, this result being effected by the chemical law of mass action. If the curve of increasing acidity is plotted against the amount of acid introduced, such a curve assumes a sigmoid or S form, rising rapidly at first, nearly but not quite flattening in the middle within the buffer range, and again rising rapidly after the buffer range has passed. But to take an isolated buffer solution as in itself typical of what happens in the body would be to commit a false abstraction. Of course not one but many mechanisms co-operate in the body to produce a level of acidity which is in practice exceedingly constant under conditions that, without the requisite organization, would make for change.

Let us return, after this example, to the general consideration of Cannon's paper. Its great importance is to have made explicit the constant aspect of animal life, the organization for constancy within the partial system that is the body. As

such it is of great importance not only for physiology but also for the study of organic behaviour. It is written with characteristic scientific modesty, and should be read by every serious student of the relations between biological and the psychological phenomena. Not only does it exhibit in much greater detail than has before been attempted the picture of the organism as a physical system tending to maintain a pattern, but it makes possible the beginning of an objective description of certain biological facts which have hitherto necessarily been described in subjective terms. I refer to those facts for which the terms " needs " and " purposes " have sometimes been used. This is not the place to repeat the arguments that have been brought forward in the last ten years to show the inadequacy of the reflex conception of biological action. In general it is claimed that some more flexible conception than the rigidly mechanical reflex must be employed to explain the highly adapted phenomenon of action " directed towards an end ".[1] Cannon's paper is a notable help towards our understanding of how a flexible conception of organic action such as seems to be demanded by the facts of behaviour may be physiologically possible. Some details are still missing, but the general scheme seems to be fairly clear.

Consider, for example, what happens if the usual supply of water to the body ceases. In this case the blood volume is preserved from the water reserves, mainly to be found in the subcutaneous tissues. If this depletion continues, changes take place in the salivary glands and the throat, and subjectively thirst is felt. " Thirst . . . become(s) more intense as the disturbance of homœostasis is more pronounced and subside(s) promptly when the disturbance is relieved." . . . " In an open system, such as our bodies represent, compounded of unstable material and subjected continually to disturbing conditions, constancy is in itself evidence that agencies are acting or ready to act, to maintain this constancy " (Cannon). One of the agencies ultimately at the organism's disposal for this purpose is locomotion of the entire system, and under natural conditions thirst is ordinarily followed by locomotion which ceases when water is found. That is to say, a thirsty

[1] For a present-day summary of the argument the reader is referred to Chapter XVII of Wheeler's *Science of Psychology* (New York, 1929). For a plea for the conception of " purpose " see MacDougall's *Outline of Psychology* (New York, 1923).

animal moves, not mechanically, because stimulated by some feature or other in the environment which produces a stereotyped response in the organism confronted with it; but it moves *until it gets water*, whatever be the "stimuli" or situation encountered on the way. It moves, we say, for the "purpose" of getting water, and because it "needs" water. That thirst and the consequent locomotion is invariably and exclusively associated with disturbance of the blood volume is not for a moment to be maintained. Too little is as yet known of the functions of the reserves, and in general of the mechanism of the whole process. But that the maintenance of this constant is intimately bound up with the thirst-water search sequence is beyond question. We may put it thus: Maintenance of the blood volume is necessary for the continued existence of the system. This maintenance is actually effected by a chain of mechanisms, which are at present not fully understood; but which includes, as a necessary link, bodily locomotion ending in the ingestion of water. Although we do not know exactly what place in the whole activity such locomotion occupies, yet it is clear that it is an essential part of the scheme.

Much the same can be said of the food search. Protein or sugar maintenance, for example, is ultimately correlated with the food search. A chain of events takes place of which an essential link is locomotion issuing in the ingestion of food. Another link in the chain we know to be contraction of the stomach, when, subjectively and in the human being, hunger is felt. This contraction is either regulated or directly initiated by hæmic stimulation [1] (Hoelzel, 1927). According to Carlson, hunger acts by increasing the excitability of the organism to external stimulation. There is definite evidence that the movements of the stomach, which in the human being are associated with the sensation of hunger, are correlated with increased organic activity, though whether or not both are determined by some prior factor does not seem to be settled. Thus Tomi Wada (1922) observed human subjects during sleep, recording both stomach contractions and the periods of restlessness. The sleeper was found to move only during the

[1] For the time being the cerebral and spinal innervation is neglected. Here participate such factors as "habit", which will be considered later. For an interesting example of the regulatory mechanism of the *milieu intérieur* see the paper of Fleisch, 1921.

most extensive movements of the stomach. In the same way Boldyreff also was able to associate the periods of apparent "spontaneous activity" of an animal with the periodically fluctuating activity of the stomach, the small intestine and the pancreas and other digestive glands.[1] This connection of phenomena to which are often applied such *ad hoc* terms as "spontaneous movements" and "restlessness" with processes which are themselves intimately related to the condition of the blood, is a striking illustration of Claude Bernard's fifty-year-old dictum. "We suppress in our explanations the internal environment, which we do not see, in order to concentrate our attention exclusively on the external environment which is under our eyes; and this enables us to adopt the false belief that there is in the living creature a vital force which violates the physico-chemical laws of the general cosmic environment" (p. 56 *Sup.*). While, then, we do not at present know in all details the part which the homœostatic constants play in initiating the periodical search for food which is so characteristic of the higher animals, we do know that these factors are as a matter of fact kept constant, and that one link in the chain of events effecting this is ordinarily a search for food. Thus we have an objective basis for the powerful "drives" of hunger and thirst, and for many phenomena which are subjectively classified under the heading of "purposive"; for, as has repeatedly been pointed out, when "hunger is felt", movement goes on until food in general is found, or salt or whatever it may be. Thus the living system is so organized that it actively tends to preserve what may be described as a number of subsidiary physico-chemical constants, subsidiary, that is, to the general integrity of the organism. These constants and the systems by which they are maintained stand in the relation of partial systems to the total system, the whole forming a hierarchy of interrelated systems.

[1] The evidence on this point has been collected by W. J. Crozier (1929).

CHAPTER IV

THE VITAL SYSTEM (*continued*)

"All the parts are affected together. Everything is in whole-part relation, and, part by part, the parts in each part operate according to their function." (Hippocrates on "Nutriment".)

Mechanical Equilibrium.—In addition there is one constant which has not yet been discussed, namely, that involving the mechanical equilibrium of the organism with the environment. It is commonly stated that in the higher animals adaptation to the environment is effected largely through locomotion of the whole organization,[1] or movement of parts of it. The mechanism by which this is effected in the higher organisms is usually called the "motor system", and is constituted of muscles which work upon the movable bones of the skeleton. The motor system receives its energy from the blood supply. By its action this aids in procuring the supplies of matter and energy necessary for the continued existence and proper functioning of the whole organism. Whether of itself it may profitably be considered a "system" in the sense of the last chapter is perhaps doubtful. Probably it and the central nervous system should better be classified as a single system; but the point is here immaterial. More important is the fact that there seems to be a mechanism for re-establishing the mechanical equilibrium of the body, when this has been disturbed by the action of the skeletal muscles. That is to say, in the mechanical equilibrium of the organism with its environment we seem to have a parallel to the other bodily constants which are preserved by a special bodily mechanism. In order to show this we must first take into account a peculiarity of the living system which has only incidentally been considered.

The Organism as Liberating Energy.—The organism has at its disposal reserves from which are liberated the energy

[1] See *e.g.* Martin, 1917, p. 37.

necessary for the maintenance and general displacements of the system. In this ability to liberate energy it is like a stop-watch or a gasoline engine or any self-running apparatus, which releases more energy than is expended to start it, and is unlike a machine that has to be worked by hand or by an outside source of power, such as the conservative mechanical systems treated in mechanics. Consider, for example, a frictionless pendulum swinging in a constant gravitational field. Granting these ideal conditions it will swing for ever in dynamical equilibrium (constant state), providing an example of a "reversible" phenomenon. Such a condition is never realized, for there are always agencies tending to bring the pendulum to rest.

But it is quite easy to arrange a pendulum that will continue in spite of friction to oscillate as long as we wish. This result is effected by adding energy to the system so as to compensate for that transformed by frictional agencies. This is done in the electric pendulum, for example, by causing each oscillation to close an electric circuit, thus actuating an electro-magnet which gives the pendulum a tug sufficient to keep the motion constant. In this way the energy of the electric cell may be utilized to maintain a dynamic equilibrium in the pendulum. The unit pendulum-magnet-cell, etc., is thus a system so organized that it conserves an equilibrium by means of energy reserves internal to the system itself. In this it offers a parallel to living creatures, for living systems are universally such that they employ intra-organic sources of energy to maintain the conservative equilibrium typical of life. Thus a mammal is kept at a higher temperature than the surrounding environment by means of the oxidation of various fuel substances within the body. "A burning candle whose wick is supplied with melted fat at the same rate at which it burns, or a benzine motor which regulates its benzine supply by means of the ball governor in such a way that its velocity remains constant, has exactly the same property as a living organism." [1]

In the same way the energy at the disposal of an organism disturbed by an external change of conditions—a stimulus—is not derived from the stimulus but is likewise released by the latter from the intra-systemic reserves, and may indeed be out of all proportion to the amount of energy involved in

[1] Ostwald, 1903, p. 24. He adds, "But we do not regard these things as living organisms, because their existence is not self-maintaining."

the stimulus. When the herd of swine "rushed violently down a steep place into the sea" one may presume that the energy consumed was far greater than that of the stimulus that started them on their flight. When Leander saw the light in Hero's window, the amount of "work" done by the ray of light upon the retinas of his eye was infinitesimal compared to that expended in the fatal swim. Such release of energy takes place throughout the whole length of the reflex arcs involved, in sensory, associatory, and motor, neurones as well as in the muscles, and is replaced from the organic stores, the process of replenishment occupying a time interval of a few thousands of a second and up.

The existence of these intra-systemic energy reserves at once raises a problem. When the stimulus, by its trigger-like action, has initiated the release of additional energy, what determines when the process will stop? [1] In order to start such a machine as a steam engine, the starting lever must be moved. A certain amount of energy is expended, a certain amount of work done on the system, with the result that a much greater amount of energy is made available through the working of the machine. But unless the engine is shut off, the process will continue until the available energy is exhausted, or until more work is done on the system from the outside to shut off the lever, that is to say, to turn it back to its original position. In the case of organic stimulation again a certain amount of energy is expended to excite the receptor and thus to release a relatively large amount of organic energy. But the difference is that this release of energy ordinarily ceases in the organism long before the exhaustion point, and without the expenditure of any more energy from without. The steam engine has to be started and stopped. The organism has to be started but apparently stops itself, at least under ordinary circumstances. It is clear that it is not enough to say that when the stimulus ceases, reaction naturally ceases. For the pressure of the engineer's hand, which is the analogue of the stimulus, may have ceased and yet the engine continues to move. A stop-watch, once started, will run down unless it is stopped by work being done upon it

[1] As we have seen, the resting organism is continually utilizing energy for the purpose of its general maintenance. This is the energy utilized in the "basal metabolism". A stimulus determines the release of additional energy which is used in "behaviour". This is hereafter assumed.

to press the button. While a child will "look towards" the source of a sound, and when the sound stops will almost immediately cease to "pay attention" to it. The stimulus will liberate energy which is used to change the child's posture, but this special process of energy release ceases almost immediately when the "stimulus" ceases.

Now it will throw some light upon this problem if we first consider certain instances where the checking mechanism has been artificially rendered inoperative, leaving unchecked the organic process of energy release. Thus according to Bethe the crab *Carcinus maenas*, when deprived of its higher nervous centres, will continue to eat until its stomach bursts. This suicidal stuffing is inhibited in the intact animal by the removed centres. Such a mutilated animal can yet distinguish non-nutritive food, which it drops.[1] It should here be pointed out, as confirmatory of what has previously been said, that the mutilated crab is still a *system*, but one which, when it is disturbed by the food stimulus, does not come to a biological or conservative equilibrium. Equilibrium, indeed, there is after the small tragedy; but not one which will be of any help biologically. Thus again from the general fact that the living system, when disturbed, comes to *an* equilibrium we can look for little help. The kind of equilibrium must be specified.

A similar example is to be found in many of the reactions called tropisms. According to Loeb,[2] the caterpillars of the butterfly *Porthesia Chrysorrhoea* are strongly heliotropic on awaking from their winter sleep. They crawl, that is to say, in the direction of light. This heliotropism has for the larva a real survival value, for it drives them to the top of the shrub which is their home, and thus enables them to find the young, new spring leaves. If, however, they are put into test tubes in such a way that the food is behind them and the light in front, they will move towards the window end and stay there until they starve. However, having once fed, the caterpillars lose their positive heliotropism: they can then creep in any direction, and consequently they move downwards from the top of the shrub to the leaves that have grown in the meantime. If caterpillars that have eaten are put side by side with those that have not eaten in two test tubes and exposed to light, the latter crawl towards the light and starve, while the former

[1] Bethe, 1897 (Hempelmann).

[2] 1918, p. 161.

Now it will be seen that the energy released by the incidence of the light upon the caterpillar was much greater than that received from the light, but also that this process of release would have continued indefinitely until either the organic energy reserves were used up or the creature was fed. In the same way the slug is, ordinarily, negatively phototropic, but the reaction is abolished by feeding the animal on cooked, but not on raw, potato. The effect is apparently due to the sugar absorbed during the digestion of cooked starch, for it can be duplicated by the injection of sugar solution.[1] During this latter experiment no attempt was made to ascertain how long the phototropism would last without feeding. There is a number of instances of this "reversal of tropism" after feeding: such, for example, is the case with certain reactions of the amœba and of the slightly more complex hydra. These examples give reason for believing that we may ordinarily expect to find provision for the checking of the continuous release of organic energy characteristic of tropistic locomotion. *A priori*, it would appear that such organisms as lacked this provision would be at a considerable disadvantage in the struggle for existence. Any animal that was heliotropic without the possibility of the checking or of the reversal of this reaction under natural conditions would ultimately share the fate of the Gadarene swine. It would end in destruction by violence or by starvation. If, in Loeb's phraseology, the caterpillar is the slave of the light, its organization must include provision for appropriate release from this bondage, or it will die.

Thus the tropisms give an example where a continuous stimulus apparently causes the continuous release of energy, but where we may, in certain cases, infer a definite mechanism for checking this expenditure. Whether an external stimulus of a certain kind is ordinarily necessary before tropistic reactions are checked, it is at present impossible to say for certain. If and when this is so, we may have an organic parallel to the case of the steam engine, which runs to the point of exhaustion unless it is actively stopped.

Almost as striking as illustrating, yet once more, the necessity in a specific case of checking the organic release of energy are certain experiments made on the jelly-fish. It is well known that this marine creature possesses on the inside of its bell

[1] Crozier and Libby, 1924.

a nerve net but no synapses, so that nervous conduction is equally possible in all directions, without hindrance by the one way transmission characteristic of synaptic systems. If now a strip of this tissue is cut out in the form of a ring, a point of which is stimulated, a contraction wave of muscle runs in two directions round the ring; if the two waves are exactly of equal strength, they cancel each other out at the antipodes of the point of stimulation. Under certain conditions, however, waves of unequal strength may be initiated, in which case the stronger overcomes the weaker pulsation and continues its path round the ring to the place of origin. Finding no obstacle to its further progress, it begins its journey again, and may be made to travel almost indefinitely round the circle without any appreciable diminution of velocity. Thus one investigator kept one of these impulses trapped for eleven days, and while there was no apparent slowing down of the speed of the wave, its amplitude did diminish owing to muscular fatigue. During this time, at an average velocity of over forty-six thousand millimetres a minute, the wave of nervous impulse with the consequent contraction travelled four hundred and fifty-seven miles. Such circuit waves do not occur under natural conditions, for the organism contains within itself the means to prevent them by mutual cancellation of waves. A similar phenomenon may be artificially produced in the heart muscle of the loggerhead turtle, and here again the organization of the heart prevents this from occurring under natural conditions.[1] Here, then, this time by the artificial employment of an external stimulus to break into an established state of equilibrium, it is again possible to induce a continuous expenditure of organic energy. A similar continuous and constant expenditure of energy is actually to be found in the higher vertebrates in the postural reflexes, which are originated in the proprioceptors of the muscles and keep the appropriate voluntary muscles in the slight state of tension which is necessary to maintain physical attitudes, though the smooth muscles of many lower forms can apparently remain in a state of protracted contraction without continuous expenditure of energy. These continuous slight contractions producing postural attitudes are entirely parallel with the reflexes by which overt movement is produced. This last instance while not so spectacular as those previously given,

[1] Harvey, 1912; Mayer, 1908.

shows how continuous release of muscular energy actually is employed in the economy of the body of the higher organisms,[1] although the probability is that a continuous stimulus is here involved.

It is now clear that the cessation of movement either with a continuous stimulus or with one of brief duration should not be taken for granted. When movement is once initiated, there is no *a priori* reason why it should stop before the bodily reserves of energy are exhausted. And yet daily experience does, of course, show that organic reaction ordinarily ceases before this stage. The point is thus stated by Sherrington, with reference to reflex action. He is speaking of the process by which, through what is called spinal induction, the excitement of a reflex tends to favour an opposed reflex. "This would help to explain how it is that a reflex reaction, when once excited in a spinal animal, ceases on cessation of the stimulus as quickly as it generally does. Such a reaction must generate in its progress a number of further stimuli and throw up a shower of centripetal impulses from the moving muscles and joints into the spinal cord. Squeezing of muscles and stimulation of their afferent nerves and those of joints, etc., elicit reflexes. The primary reflex movement might be expected therefore of itself to initiate further reflex movement and that secondarily to initiate further still and so on. Yet on cessation of the external stimulus to the foot in the 'flexion-reflex' the whole reflex comes usually at once to an end. The 'scratch-reflex' even when violently provoked, ceases usually within two seconds of the discontinuance of the external stimulus. . . .

"We have as yet no satisfactory explanation of this. But we remember that such reflexes are intercurrent reactions breaking in on a condition of neural equilibrium itself reflex. The successive induction will tend to induce a *compensatory* reflex, which brings the moving parts back again to the original position of equilibrium."[2] Here again the fact is noted that, once initiated, a motor response might be expected to persist indefinitely, and a hint is given as to the mechanism by which this biological absurdity is avoided. In this case,

[1] L. S. Kubie has made an ingenious application of the concept of such closed circuits. His hypothesis, which has Sherrington's commendation, is that such "trapped waves" may occur in the human cerebral cortex with pathological results (1930).

[2] Sherrington, 1911, pp. 213–4.

as in that of the jelly-fish, the danger of "organic perpetual motion", lasting until the energy reserves are exhausted, is apparently due to a peculiarity of organization which brings it about that the completion of one cycle of energy release ostensibly sets the stage for the initiation of the same cycle. The nervous impulse travels round the ring until it reaches its place of origin; the reflex, by means of the internal receptors in the moving structures, apparently provides by itself the stimulus for a further reflex; and in each case there must be assumed to exist in the organism a specific mechanism to prevent such circular action.

Mechanism of Checking Energy Expenditure.—But apart from special mechanisms made necessary by such cases, there apparently exists a quite general provision for the checking of response to a stimulus of brief duration. Many factors seem to contribute to the effect. For example, mechanical conditions often do not allow of protracted *movement*. When we hear a noise we cannot turn our head through more than a certain angle. Yet here, of course, it would be possible for us to continue to expend energy by keeping the neck twisted indefinitely. This again we do not do. Aside, however, from the possibility of such mechanical and other similar brakes, the most important factor in checking organic motion is to be found in the conservative changes which take place throughout the body after the stimulus has ceased, and which in effect do for the reacting organism what the engineer does when he shuts off the engine.

For not only does the organism as a whole reach a conservative equilibrium after disturbance, but the same result is apparently achieved in every structure affected by stimulation. If, after the stimulus has passed, the parts affected were again brought back to the original pre-stimulus state, then, external conditions being as they were before, the outgo of energy would subside to the pre-stimulus level. This is what actually appears to happen.

Consider the sensory-motor arc before stimulation. Here is a chain of structures all in mutual equilibrium with each other and surrounding conditions. When the receptor is stimulated by the incidence of a very small amount of external energy, each structure of the chain is progressively disturbed, until the muscle is finally activated, releasing energy by its contraction. The recovery process of the nerve has been

extensively studied, and the name "refractory phase" given to the period of time during which it is incomplete. There are several stages in the process: first the absolute refractory phase, during which no impulse can pass, secondly the relative refractory phase, during which impulses cannot pass at first but succeed only in restoring the absolute refractory phase, while, later, impulses can pass but at a sub-normal intensity. Lastly, there succeeds a "supernormal" phase, during which there pass impulses of an intensity greater than normal. After a very brief interval of time, the nerve is then in approximately the same state as before the stimulus, and ready to conduct a second impulse. Throughout the neural part of the chain compensatory processes have restored the pre-stimulus state.

At the same time there has apparently taken place a similar process of recovery in the receptor. Let us take a simple example. A frog, let us say, is fastened to a frog-board with its legs hanging free below the wood. I pinch the muscles at the top of the leg, when both legs are drawn up and then dropped again. The reflex arc was originally in equilibrium; by my pinch I have disturbed this equilibrium and the legs move. But when the pinch ceases, the receptors are obviously not in the same state as they were during the incidence of the stimulus. They are no longer subject to the same deformation, because the physical structure of the muscle is such that it "tends to retain its shape". That is to say, in this most elementary of all ways, by the sheer mechanics of its physical structure, the organism tends to reinstate the pre-stimulus condition, thus beginning the chain of restorative processes which re-establish equilibrium. With such elementary restorative action may be compared the operation of such a structure as the electric bell, on which a certain amount of work is also done to initiate the release of energy, and which ordinarily contains a mechanism in the push-button—the receptor!—tending to restore it to the pre-stimulus position. If this spring breaks, then pressure on the button will cause the liberation of energy until the batteries run down. It is, of course, probable that other restorative processes are initiated in the frog's receptor besides the sheer release from deformation.

Provision for such receptoral restoration is apparently made in a number of other ways, the details of which are certainly

not all known as yet.[1] It is, however, possible to distinguish the cases where the process is of a mechanical nature, as in the case taken, from those in which it is chemical; though at times both methods are apparently employed together. In the ear, for example, there is mechanical provision for the purpose. The mechanical apparatus as a whole is brought to rest in the pre-stimulus position by the elasticity of the frictionally impeded membranes. The problem here presents itself in a special form, as that of providing a means whereby the oscillation of the tympanum shall be caused to cease as soon as possible after the physical stimulus is over. Thus in the outer and the middle ear the ossicles load the drum in such a way as to form an effective damper, allowing the maximal freedom of vibration with the minimal after-vibration on cessation of the "sound". Thus auditory impulses are initiated and checked by a mechanical system in highly stable equilibrium. This system is of such a nature that after disturbance by an auditory stimulus it rapidly comes to rest at the pre-stimulus state, the whole neuro-mechanical apparatus, as depending on both mechanical and physico-chemical principles of action, illustrating the complex nature of the highly intricate system that is the body.

Equally interesting but much more complex is the case of the organ of vision. Here certain aspects of the swing back to a pre-stimulus state have been experimentally examined, among them those connected with the so-called photo-electric properties of the eye. If a recently excised eye is placed in a circuit with a galvanometer, contacts being made with the cornea at the one end of the eyeball and the cut optic nerve at the other, there is found to flow through the wire a current which is sufficiently large to affect a delicate instrument. This is the well-known "current of injury". Instrumental adjustment is so made in the experiments to be described that, in spite of it, the galvanometer reading is zero. If, then, light is allowed to fall on the base of the eye, the galvanometer-needle is deflected. If the light is removed the needle returns to zero. Figure 1 shows the result of one such experiment, exhibiting the swing back to the original condition after the illumination is removed. Figure 2 shows a record made of a series of stimulations. The return of the eye towards its

[1] Adrian and Zotterman give evidence tending to show that in certain cases receptoral restoration is slower than its neural counterpart.

normal electrical condition is clearly seen, though a general tendency will be observed for recovery to fall somewhat short of the pre-stimulus state. Whether this is due to " fatigue " or to some other factor is uncertain.[1]

We must think of the retina with the nervous appendages as forming together a system, partial, of course, to the total

FIG. 1.—Electrical Changes produced in the Retina by Light.

Figure 1 shows the electrical responses to light in the rabbit's retina. The white area represents a period of illumination lasting for half a second, the black areas periods of darkness. A pronounced electrical response is caused by the light stimulus, approximate equilibrium being reached towards the end of the half second period. The subsequent darkness is followed by gradual relapse to the original state. " Apparently darkness causes a temporary effect of the same sign as light does." (From Bayliss after Piper.)

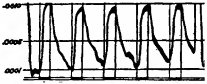


Figure 2 shows the electrical responses to light in the frog's retina. The photogram shows five normal responses due to a candle at two feet with illumination for one minute and obscurity for two minutes, the time in minutes being plotted horizontally and the absolute electromotive force vertically. Rapid rise in electromotive force is shown with, on the whole, less rapid recovery. Recovery is evidently not complete, since there is a constantly rising minimum electromotive force at the end of each two-minute period of darkness. (Sheard after Waller. L, light, D, darkness.)

system of the body and to the environmental situation, but yet retaining a measure of separateness and individuality of its own. In this system there are occurring, in a manner

[1] Sheard, 1921.

conditioned by the organization, certain electrical displacements which, as the eye recovers from the shock of the operation, gradually reach a stationary state, that is, attain an equilibrium. When light falls on the retina, the systemic electrical displacements are disturbed; when the light ceases to fall on the retina, the system returns almost to its previous state.[1] But the incidence of the light must have effected some change in the retinal substance, so that we must make what we know from other considerations to be a reasonable supposition, namely, that on the removal of the light there is a change of the light-affected substance or substances back to the "normal" state, corresponding to the "normal" or nearly "normal" state of the electrical displacement.[2] There is definite evidence of activity in the eye after the removal of a stimulus, as shown by the results of Adrian and Matthews,[3] who found a renewed outburst of nervous impulses on the extinction of a light that had lasted more than a second. This phenomenon was termed the "off-effect", and it was suggested that its likeness to the terminal rebound in a reflex contraction is close enough to suggest a common origin.

The experiments of Hecht (1930), to be considered in a later part of this book, throw still further light on the process of restoration in the eye after disturbance of equilibrium by a light stimulus. Briefly, it may be said that Hecht regards the visual stimulus as supplying the energy whereby a certain amount of a specific chemical is released. This chemical in its turn initiates a reaction which sets off the nervous impulse. The precise method by which this second chemical action is set off is apparently uncertain; but if it is by combination with the light-liberated substance, then we may have the

[1] "We may suppose that (there are) substances which in the absence of light are ionized to a degree dependent upon the equilibrium constant of the reversible dissociation action (*i.e.* of the photo-sensitive substance in the eye). The introduction of light into the system alters the value of this constant." . . . (Sheard, p. 107.)

[2] See Köhler, 1920, Chapter VI, for a brilliant discussion of ocular physics, with interesting psychological corollaries. Titchener states that "to account for contrast, we have merely to suppose that the retinal substances tend towards equilibrium over the whole area of their distribution" (1915, p. 91). The existence of after-images testifies to the after-effect of the stimulus, the negative after-image probably corresponding to a retinal compensatory process.

[3] 1928.

picture of the light stimulus liberating a certain quantity of a chemical substance, which is sufficient to initiate nervous action and which is used up in the process.

This excellent work of Hecht's, accepted by the majority of psychologists, though not by all, greatly clarifies the picture of equilibrium-restoration in the eye. In general the process is one whereby a photo-chemical system, partial to the total organic system and also to the external photic conditions, comes to a constant state when these latter conditions are kept constant. When the photic conditions are altered, the photo-chemical system must correspondingly alter, and in so doing will stimulate the nerve and initiate the processes accompanied by vision. When the photic conditions again swing back to their original state, the photo-chemical system likewise regresses to its original condition. While in the case of the ear the mechanism for restoration was largely mechanical, here it is chemical.

We have spoken of restoration to the pre-stimulus state in receptor and nerve. Similar processes are known to take place in the muscle.[1] Thus after the alteration of external conditions has disturbed the equilibrium of the receptor, ultimately initiating overt reaction, then if, as generally occurs, external conditions return to normal, the whole sensory motor arc has also returned to "normal".[2] Under these conditions there is equilibrium throughout the arc, and between it and the environment, and release of energy is checked.

We are then able to give a general answer to the problem of how additional energy output, once initiated by a stimulus, is checked before the exhaustion point. The most general answer is that the body is a system in equilibrium towards external environmental changes or stimuli. After disturbance, this equilibrium is restored by a special organization, by means of which the bodily structures, specifically the receptors, nerves and muscles, are brought back approximately to the pre-stimulus state. By this means continued liberation of additional energy is checked, as in artificial machines with a checking mechanism such as the electric bell provided with a push-button. At the same time the whole sensory-motor arc is prepared for further stimulation. The total process

[1] Bayliss, 1924, pp. 446, 454.

[2] The word is used without implications, and as equivalent to "pre-stimulus".

is an excellent instance of Haldane's fundamental biological postulate, that the organic identity of a living organism actively maintains itself in the midst of changing external circumstances. Energy-liberating machines may be classified as those such as the steam engine which, once started, require the expenditure of energy from an outside source to stop them, and as those which, like the ordinary electric bell, include an automatic checking mechanism, whereby the whole structure, after disturbance, automatically reverts to the original state of equilibrium. The body belongs to the second class.

It is by means of this mechanism for checking the additional energy output consequent upon stimulation that mechanical equilibrium with the environment is re-established. As already stated, the mechanical equilibrium of the body is thus on a par with the other bodily constants which are preserved by means of the special organization of the body. The mechanism for checking additional energy outgo is at the same time the mechanism for re-establishing mechanical equilibrium.

The Oculo-motor System.—Now Köhler has pointed out that if we are to speak of electrical processes in the eye we must take into account the electrical condition of the whole of the relatively isolated system in question. Electrical phenomena occurring at different points cannot be considered as independent, "additive", but must be treated in relation to the totality of such events in the whole unit under consideration. Apart from general theoretical considerations, such as the Gestalt experimentalists have so ably adduced, certain researches now make it clear that neural events in the eye cannot be considered as the sum of separable processes in isolated neurones, but that these neurones function together as a unified whole. Adrian and Matthews have lately made an experimental investigation of the effect of light upon the eye of the conger eel, using the "action current" of the optic nerve to detect the nervous changes, and employing radio amplification to render them observable. Their data show that "retinal neurones in the eel's eye may become so closely coupled that they work in unison. . . ." We must regard one or both of the synaptic layers (of the retina) as a sheet of nervous material capable of acting as a whole (Adrian and Matthews, 1928). "The eel's retina", say these workers, "is not a mosaic of receptors or groups of receptors

with each group leading by an independent pathway to the corresponding optic nerve fibre" (*ibid.*, p. 294). The interdependence of retinal elements is confirmed for the human eye by R. Granit's experiments (1930). Still more striking evidence of whole-processes occurring at the pre-neural stage is given by Hecht's (1930) whole series of researches already mentioned which treat the process of visual excitation as a unity, and elaborate a formula for the chemical state of the excitatory system-as-a-whole. Caution should be exercised with reference to the theoretical implications of the experiments. But at the present time they seem to present a confirmation of the hitherto theoretical conclusion that the processes occurring in the eye, whether neural or pre-neural, are to be treated as wholes, not as the linear sum of discrete excitations. The retina and its associated structures must then be considered as partial systems within the total organic system. At each successive level we have a complex within a complex, formed by the specific interrelation of certain elements of the total living system, apart from those connections which constitute them part of the total system. As part of the total living complex a retinal cell is related to all other cells of the body; but, in addition, it stands in a new and specific relation to other retinal cells. It is these *retinal* relations which are not shared by the rest of the body that give a special unity to the partial system in question. Indeed, the visual receptor-apparatus apparently functions as a hierarchy of systems, each partial to the next above it and to the whole visual system, which in turn is partial to the total living system, which, again, has been seen to be partial to the environment-organism complex, and ultimately to the whole universe.

When the retino-chemical system is thrown out of equilibrium by a visual stimulus or change of photic conditions, a disturbance is apparently propagated through successive layers of neurones, with release of energy ultimately reaching the cerebrum. One of the usual effects of such disturbance is motion of the eyes themselves, which will next be considered.

Now bodily movement, as we have seen, may be divided into two categories, bodily translation of the whole organism or locomotion, and movement of part of the organism relative to the rest. Our attention hitherto has mainly been directed towards the first class, although mention has been made of

reflexes which characteristically involve movement of one limb or structure relative to the rest of the body. One of the most interesting instances of the second or relative class is to be found in the movements of the eyes. As is well known, each eye is moved in its socket by six muscles external to the eyeball, two turning the glance up and down respectively, two to the right and left, and two rotating it. According to Koffka these muscles, and the branches of the three cranial nerves that innervate them, together with the retinal sensory structures, the optic tracts and the connecting centres are to be considered as a single system, which is relatively isolated from the rest of the body, and forms a functional, interrelated unity, in our terms a specifically organized partial system (1924, pp. 80, 81).[1] "Assume an infant lying on its back in a totally darkened room, . . . and allow a light to fall on the peripheral region of its retinæ. The infant's optical system will then be in a state of disequilibrium occasioning eye movements which continue in a certain direction until equilibrium has been re-established. This will be the case when the light falls upon the fovea of each eye." Here is a different kind of equilibrium from any yet considered. The six external muscles of each eye form together a mechanical system, which, when the retina is in darkness, comes by the mutual interaction of the tonus of the muscles to a state of equilibrium with the eyeball turned slightly up and inwards. Mechanically then each eye and its external muscles form a separate system, and we may speak of the constant state thus reached as the position of mechanical equilibrium. But if light falls on the retina, the structure is no longer functioning as a mechanical, but rather as a photo-mechanical system. Under these circumstances its conditions of equilibrium are different; to take a simple case, if a spot of light falls on the retina in a darkened room, equilibrium is attained when the light falls in the centre of the eye on the fovea, or point of clearest vision. Mechanical and photo-mechanical equilibrium of the eye may then be distinguished.

Now as the result of certain theoretical considerations Koffka makes the further assumption that if light falls on any point but the fovea, thus disturbing equilibrium, intrasystemic forces are progressively aroused that move the eye in the direction of photo-mechanical equilibrium. It may

[1] Cf. Köhler, 1920, pp. 201–2.

easily be seen that something like this proposition would be true of the mechanical equilibrium of the eye. If, when a person were lying in the dark, one of his eyes was somehow displaced by mechanical means, as by twisting with the fingers, the tension of the external muscular system alone would restore the position of mechanical equilibrium. Thus systemic forces would be progressively aroused which would tend to bring the eye to the resting state. Stated otherwise, there is one position only for this mechanical system which will not be followed by motion. Koffka's further assumption is that for the photo-mechanical system of the eye, there is similarly one position only on which light may fall without exciting motion. When light falls on the eye, movement then naturally takes place until this position is reached. It must be remembered that this process involves the co-operation of retina, nerves and muscles, and that the equilibrium or constant state is thus one involving not one but several structures and kinds of energy, and is thus more complex than any hitherto considered. Neural organization of a kind hitherto not discovered must also be involved. The hypothesis thus has its difficulties. Its originator makes, however, a very good theoretical case for it, and it has, in addition, the support of certain experimental findings.

One of the most important of these is given by certain experiments on the transposition of the external eye muscles. Suppose that the point of application of one external muscle were changed; the movements towards equilibrium consequent upon mechanical displacement would be little affected.[1] That is because the muscles work together as a whole, a system, to restore equilibrium. Such a transplantation was claimed to have been effected in the eyes of a number of apes under the direction of Marina (1915). For instance, the *rectus internus* muscle was eliminated and replaced by the *obliquus superior*, and in another experiment by the *rectus externus*. Thus the muscle whose normal function is to rotate the eye towards the top of the nose, now turns it pupil and all towards the nose, if its action is considered in isolation; and similarly in the second experiment the muscle previously rotating it away from the nose now has the opposite effect. Considering them in isolation the individual muscular connections have been

[1] There might be a slight squint produced, as happened in certain of Marina's experiments.

interchanged. Yet the total action of the system was found to be unaltered. After recovery, the position of mechanical equilibrium in the eyes of the animals was practically unchanged, though slight squints were in some cases observed, due in one instance to faulty technique.[1] This result is easily understandable from elementary mechanical principles. But one is not so easily prepared for the fact that the eye movements occurring after visual stimulation seemed to be equally unchanged. But this fact, which the experiments seem to show, is in agreement with Koffka's assumption that just as the external muscles of the eye, considered as mechanical agencies, together form a mechanical system, so the photomechanical structures of the eye form, together, of course, with the nervous connections, a photo-mechanical system, displacement of which is followed by forces evoked by the system as a whole, and which tend to bring back the system to its position of photo-mechanical equilibrium. The instances given here are purposely simplified; for a fuller account the reader should consult Koffka's work, with the references he cites.[2]

If this theory is correct—and it must be admitted that it is hard to reconcile with current neurological opinion—then in eye-movements we have yet another instance of the body's power to restore a constant state, and one really in line with Cannon's theory of homœostasis. For just as it is inconceivable that there should be separate nervous connections serving to restore a normal temperature after developments of heat that would be expected to raise or lower this temperature one, one and a half, two, two and a half degrees and so on, so Koffka's theory claims that separate nervous connections are not employed to bring the eye to a position of equilibrium when it has been disturbed by stimulation of different retinal areas.

Levels of Equilibrium.—The fact that the gaze may come to rest with the eyes in different positions relative to the rest of the body calls attention to the possibility of different states

[1] The results of the two sets of experiments are here combined. One may compare with Marina's experiment the paper of Robert Kennedy (1901), where similar results are reported for other parts of the body.

[2] It is to be wished that certain further details had been given in the account of these interesting experiments. In fairness, however, it should be stated that the experimental result was *confirmatory* of Koffka's general hypothesis which was made necessary on other grounds. Nevertheless, the experiment undoubtedly deserves repetition.

of equilibrium in the same (partial) organic system. It is common knowledge that during fever or, in health, at different times of the day, the temperature-regulating mechanism may maintain body-heat approximately constant at an entirely new level. In this case, it is natural that attention should be focussed at first on the fact of constancy rather than on the intervening displacements by which constancy is attained; the thing that has generally struck observers is that the temperature is kept constant at these different levels, rather than the behaviour of the body when the temperature is shifting. This is perhaps because the changes are apparently of much shorter duration than the periods of equilibrium. On the other hand in the case of eye movements attention is naturally directed to the movements or shifts of equilibrium as the most obvious part of the phenomenon. This difference of obviousness, as it may be called, tends to obscure the essential similarity of the two phenomena. If Koffka is right, we have seen that there is a direct and fundamental parallel between the attainment of a fresh temperature level by men on the march and the attainment of a fresh state of rest for a few moments by a baby's eye when the beam of light from a candle falls on it. On the hypothesis stated, each set of events depends on the re-establishment, by a progressively working mechanism, of a new constant state. In each case, of course, the new level is maintained by the continuous expenditure of bodily energy, the temperature by means of general oxidation of fuel reserves, the ocular fixation by the liberation of muscular energy.

The existence of these different levels of equilibrium creates a certain difficulty, the result of which has been that the phenomenon in question has often been given a teleological explanation. This may be typified by a quotation from St. Paul most appositely made by Barcroft (1925, p. 178), who is showing in his account of acclimatization to high altitudes, that various bodily constants which are very exactly maintained in ordinary life, are in times of stress shifted to a new constant level. I quote the whole passage:

"It is no part of my philosophy to suppose that this or that function of the body is kept constant by a process of sweating its fellows, and as far as I can see such a doctrine is quite unsupported by facts. The oxygen in the arterial

blood does not remain constant, the CO_2 pressure in the alveolar air (*i.e.* in the lungs) does not remain constant, the hydrogen ion concentration (acidity) of the blood does not remain constant,—but they and a score of other things settle down to form a new equilibrium. If artificially one is altered, the rest alter in unison. . . . This is no new doctrine. It has been stated in words that have come through the ages. 'If one member suffer all the members suffer with it. The whole body, fitly joined together, and compacted by that which every joint supplieth according to the effectual working in the measure of every part, maketh increase of the body to the edifying of itself.' "[1]

It is very tempting here to follow the almost universal practice of the last two thousand years and think of the bodily constants as altering for the purpose of conserving the body. Barcroft is, of course, careful not to imply any such thing. The temptation to such teleological description is, however, lessened if it is remembered that we have here to do with a number of partial systems, themselves highly organized, but standing also in a definite relation to the rest of the body and to the environment. Change of environmental conditions such as those caused by high altitude thus cause change in the functions in question, and if we knew all the factors involved it would be possible to state a set of constant relationships connecting the systems. In the same way, the appropriate total organization causing constancy of the organic factors in question keeps them constant at the new level. Because, however, the body is apparently constant while these functions vary we are apt to think of the body as "prior" to them, which, indeed, it is in the sense that the total system is logically "prior" to the partial system. Such logical priority must not, however, be allowed to bring with it teleological implications.

Multiple Control of Organic Partial Systems.—In the second chapter it was seen that a partial system with its own specific organization may be considered to be subject to two sets of controlling conditions, namely, those that arise from the internal organization of the partial system, and those which spring from its relation to the total system of which it forms a part. Thus the behaviour of the spring in a spring balance is affected by the fact that the molecules composing it together

[1] The words in parentheses are inserted by the present writer.

form a spring, and also by the fact that there is a certain weight on the balance. Ultimately of course this distinction is a logical one only, but it does form a convenient means of classification, and is in fact implicit in the thermodynamical treatment of heterogeneous systems.[1] In the organic system this dual control of the partial system is replaced by a highly complex multiple control. Here we find an intricate enlacement of unitary partial processes, each of which is related to the other, and generally to conditions obtaining outside the organism. This is illustrated by the quotation just made from Barcroft. Specifically, Henderson (1930) states that the blood system involves twenty relations between six different constituents. Each of these twenty equilibria thus forming part of the total blood-system equilibrium, which again forms part of the total living system, will change under two distinct conditions. If for any reason a shift occurs in one of the other nineteen equilibria, a change must take place in the twentieth. If a change occurs in the external conditions, there will likewise occur changes in the blood equilibria. Of course, external change in altering one of the related blood equilibria will therefore alter all the others simultaneously. Thus change of altitude will directly alter the pressure of the alveolar carbon dioxide in the lungs, and as a result the twenty blood equilibria will all change. The equilibria in which carbon dioxide plays no immediate part such as, according to Henderson, that involving barium chloride, oxygen, and acidity, will then be shifted. Each of these interlaced unitary systems is then unified first by its own internal set of relations; it is also related to nineteen co-ordinate systems, within the body, and also to the external vital conditions. Thus these partial systems of the body, if their existence can be considered to be established,[2] own a multiple control. They may be altered by alteration in any one of the co-ordinate systems and also by change in the environment. To speak of a multiple control is, as hitherto remarked, a logical device only. But it is one the fundamental principle of which Henderson has apparently found useful in the description of a typical set of organic events.

[1] Where the "liquid phase," etc., is considered in relation to the total system.

[2] I understand that the physiologists have not yet made up their minds about them. F. S. C. Northrup's account is adopted.

Leaving Henderson's work for the decision of posterity, we have to hand a much simpler example of the multiple control of partial systems in the body. This is to be found in the ocular movements already considered from a slightly different point of view. A child " looks at ", turns the eyes towards, a lighted candle in a dark room. The equilibrium position of the eye is then affected by the events of the environment. This is what we have called the " external control ", although it is of course mediated by the internal organization of the oculomotor system. But more than the visual field must be taken into account in deciding the equilibrium position of the eyes. The " voluntary " movements must also be considered, which of course originate in some organic source external to the oculomotor system. The oculomotor system is part of the total bodily structure. The eye is part of the man and may be affected by organic conditions as well as by what is called for by photic environment. It owns again more than one master. Thus, since the pioneer work of Dodge it has become general to distinguish five types of eye movement (Dodge, 1902). There is the type already considered, namely, that which is a response to eccentric photic stimulation. This type of movement cannot be influenced as regards its velocity by voluntary effort, a rather surprising conclusion, which it is difficult to induce a student to believe, when he meets it for the first time. Further, during eye movements of this type vision is practically suspended ; [1] they are really interruptions of vision for the purpose of shifting the line of regard. Different are what we have learned to call pursuit movements. Here the eye follows continually an object moving in the visual field, such as a moving candle. Such movements differ from those of the first type in that they are continuous, may vary in speed, and are accompanied by clear vision. Hypothesis would be premature, but one may be fairly certain that this type of movement takes place under different neural conditions, and one would seem justified in suspecting a more highly integrated cortical relation. In the same way, during those movements which are made in compensation for motions of the head and the body, the eyes are clearly acting in relation to the total organic system, while

[1] Experiments subsequent to Dodge's study seeming to show that there is partial vision during such eye movements.

in movement of convergence we apparently have again the visual field participating.

Thus the eye is under multiple control. Sometimes it may be detected taking orders from one master while it is relatively neglecting the others. In a later paper Dodge has shown us a case of antagonistic control, and has exhibited what happens in case of conflict between the semicircular canals, which cause ocular movements during rotation, and the visual apparatus. The eye is seen alternating between the two controls, responding first to the vestibular, then to the visual influence, which again may be seen from the records to give way to the vestibular control and so on. These beautiful records will repay study in the original by all who are interested in the mechanism of the body (1923*a*).

According, then, to their relations with the other structures of the body and the environment the organic partial systems may come to different positions of equilibrium. The temperature may rise above "normal" if different conditions obtain in the body outside it; the eyes may come to different states of rest, either directly according to external conditions, or mediately through the influence of other bodily structures,[1] and so on. As interrelated partial organizations of a more inclusive system, which is itself partial to a still more comprehensive one, the organic sub-system is under control from many sources. This conception, in a somewhat disguised form, has been found of practical value by those engaged in the experimental investigation of living processes.

Weiss' Experiment.—Mention has already been made of Weiss' brilliant application of the systemic conception to the behaviour of the butterflies *Vanessa Urticae* and *Vanessa Io.* Weiss is a biologist of considerable reputation and has used the notion of the living system to make a direct quantitative experimental attack on a problem of behaviour. He defines a system as a complex tending to preserve a certain constancy

[1] In spite of this variation, we have, it has been pointed out, the sanction of logic in speaking of the same partial organic systems at different equilibria. The obviously constant anatomical structure of the eye helps out the logical point. The reader is again reminded that, strictly speaking, an isolated mass of gas at, let us say, a temperature of 400° is a different system when heat has been added sufficient to raise the temperature to 500° C., keeping the other factors of equilibrium unchanged.

in spite of change in one of the elements of the complex.[1] He points out that, granting such a tendency, then change in the one part must be followed by compensatory changes of the other parts, such changes ceasing only when equilibrium is re-established and constancy thereby restored. Such a position of equilibrium will then, he says, be a limiting, that is, a maximal or minimal one, for the transitional states will continue until the time when "naught occurs, for nothing can occur", all the forces concerned being equally balanced. An example from the inorganic sphere is to be found, again, in the pendulum, which moves as long as any of the operating forces have, so to speak, an advantage over the others; under ordinary frictional conditions movement then takes place until there is no reason why it should occur in one direction rather than another, that is to say until the vertical or minimal position is reached and momentum no longer operates. This argument has already been considered, in the last chapter.

This general conception Weiss applies directly to the consideration of the equilibrium of living creatures. The butterflies in question *Vanessa Urticae* and *Vanessa Io* perform certain characteristic movements when they are placed in the light in a tired state. The movements follow automatically and necessarily one after the other, and fall into three divisions. There is first a movement upwards, which is effected either by creeping or crawling. This is followed by the assumption

[1] I give herewith a literal translation of Weiss' general statement:

1. As System we define herewith a complex which exhibits the tendency (das Bestrebung zeigt) to remain constant as a whole towards the external wor d when its parts are altered.

2. In this tendency towards the maintenance of constancy it goes for granted that the state of the system represents the stable, that is to say, unique, state of the complex under the totality of the external conditions.

3. The maintenance of constancy in the whole when the parts are altered can only be brought about by compensatory alterations of the other parts, which last alterations the system itself thus effects (systemic reaction). Every disturbance of the stable state, every deviation from the point of unique determination, is followed by alterations which persist until the attainment of a fresh state of unique determination, that is to say, a state in which "nought occurs, for nothing can occur." . . .

4. The state of the system must be characterized as a special (unique) state because of a limiting condition (maximum or minimal condition) of the operation of the factors involved.

(*Loc. cit.*, p. 183; the fifth statement is not quoted.)

of a position dictated by the force of gravity, the butterfly turning on a vertical wall through 180° so that its head now faces the ground, and remaining at rest in that posture. Finally the creature takes up a position with reference to the illumination. When there is one source of light only, and the surface is horizontal, so that the influence of gravity is eliminated, the butterfly places itself symmetrically with reference to the light and with the head away from the illumination. When there are two lights, the position is such that the eyes are equally illuminated with the minimal amount of light-energy falling on them. It was demonstrated that the light had no effect on the gravitational position, so that the two external agencies may be separated. If, Weiss argued, the butterfly acted as a unified system towards gravitational force, it should take up a symmetrical position when it is coming to rest on a vertical surface under the action of gravity alone. That is to say, it will continue to move until there is no reason why it should move in one direction rather than another, which will, of course, ultimately result in a symmetrical posture. But such symmetry is possible in two ways, namely, in the vertical or the horizontal position. Now by an ingenious mathematical analysis of the butterfly's legs and body, considered as a mechanical system of linked rods, Weiss is able to show that, of all possible positions that can be taken up on a vertical wall, that actually adopted by the butterflies when they turn and come to rest with their heads hanging down is the most economical, in that it is maintained with the least energy possible under the particular circumstances. "It is noteworthy that the limiting condition actually found is not fulfilled by the mechanical peculiarities of the system. It is not simply a question of the automatic movement of the centre of gravity to a position as low as possible, as is the case with mechanical systems; but just as the conditions are physiological, so is also the manner in which they are fulfilled. Work is done in order to come to a minimal position in which work is later economized." This result is then in harmony with the general conception of the butterfly as a physiological system which so acts towards the external gravitational force that it tends, in rather a refined manner, to preserve its "general bodily integrity". It places itself in such a posture that the least possible energy is expended while it "sleeps". Such a position is a limiting or symmetrical

one, as is also the position taken up towards the "light" stimulus. It is further a state of biological rather than one of mechanical equilibrium.

Weiss' general position is then more or less that adopted in this chapter. He considers the living creature to be a system so organized that it actively maintains its identity in and contrasted with the environment. This is the same thing as to say that events in the history of such a system will tend to exhibit a definite "direction" towards the preservation of the organism, which again is perhaps a paraphrased statement of belief in some form of an "instinct of self-preservation". Even at risk of repetition it is, however, worth while to point out certain divergences between Weiss' conclusions and those of this book. The present book makes the distinguishing mark of a system the interconnection or interrelation of the elements. According as these interrelations are mechanical, chemical, or physiological, and so on, the system is described as a mechanical, chemical or physiological system. Certain systems have the property that, when they are disturbed in certain specific ways, compensatory changes tending to restore the *status quo ante* are initiated within them. This property is due to a special organization which may be different for each system. According to their specific organization, different systems may initiate compensatory adjustments for different kinds of disturbance. Thus the pendulum hanging in air is so organized that it tends to restore the vertical position, but it is not so organized that it tends to restore its temperature when this is disturbed. To effect this, another type of organization is necessary, namely that possessed by the "constant temperature chamber", or less perfectly by a chemical system obeying le Chatelier's rule. It is the view of the present book that when, considered from the physical side, a living organism is a system so organized that it can actively maintain itself or more exactly its pattern against a very large and very diverse number of external disturbances, and that as evolution advances this property of self-maintenance correspondingly develops. The property of active conservation is thus considered in this book to be superadded to the system; while, according to Weiss, it constitutes the system. Of the two definitions the one adopted in this book appears to be the more general, for it includes all complexes however they

are interrelated; while Weiss' definition includes only such complexes as have the self-conservative organization. Weiss' definition has the advantage that it can be immediately applied to living systems, but has the disadvantage, as has already been pointed out, that it is hardly consistent with the use of the term in physical science.[1]

Weiss' important paper seems to raise a further difficulty. This concerns the notion that organo-systemic action necessarily tends towards a limiting state.[2] Such an assumption is, it has been seen, a postulate of physical science for physical systems. Unless an organic system, after disturbance, actually came to rest or " equilibrium ", that is to say, to a " constant ", which is a " limiting " state, there would arise the phenomenon of " perpetual motion ", the impossibility of which is a basic principle in physical science. This is all pointed out in Weiss' paper. There are, however, notable differences between the physical and the biological system, which make doubtful whether the existence of action towards a limit can be directly inferred in the latter. Weiss' argument applies only to a theoretically isolated system; the biological system is essentially a partial system. Weiss' argument concerns overt

[1] As previously stated (Ch. II) Weiss' definition possibly implies the writer's. For a system, it may be said, must oppose a certain constancy to actually changing conditions. Otherwise, it will instantaneously dissolve whenever external conditions change. Whether, in that sense, one should say that in the last analysis a physico-chemical system is so organized that ultimately some transcendental quantity will remain constant I am not prepared to say. Such a constant may be a necessary figment of thought, which finds it essential in the last resource to assume a permanent in change. At least for mathematical treatment, as Poincaré points out, a general equation is necessary, which implies some kind of constant relation. In any case, I have found it has clarified my own thinking to adopt the more general definition of the system, at the same time pointing out that *specific* interrelations may result in a specifically added property of constancy towards some particular external change. I apologize for repeating this important point.

[2] In his interesting paper, " Gestalt Psychology and the Philosophy of Nature," *Phil. Review*, November 1930, Dr O. L. Reiser calls attention to an article of Professor C. D. Murray, in which the latter " by developing the idea that ' physiology is essentially a problem of maxima and minima ' tries to show that the cost of operation of physiological systems tends to a minimum when measured in units such as the calory and the erg. This has been declared to be a misapplication of the principle by Professor P. S. Bauer, who argues that, in general, equations which

movement. It is not immediately apparent that, in a mixed system such as the organism he investigates, and which comprises mechanical, chemical and thermal transformations, the limiting state of the total system should coincide with a limiting and resting position of the externally observable structure. For example, since energy must be expended at all times it would seem possible for the limiting position to be reached with the mechanical system in continuous motion, such as a continuous slight movement of the wings. It is with some uneasiness that these considerations are advanced, in view both of Weiss' important contribution and of the fact that his arguments apparently stand the pragmatic test of experimental verification. For in spite of theoretical objections, the organism does behave according to Weiss' canons.

What is observed is that the living system actually does take up a minimal position with the result of ultimately saving energy, and before the energy reserves are used up. But to explain this it seems hardly possible to appeal to general principles; we must postulate that this economical behaviour is due to a special organization of the living system in question. That is to say, the phenomena observed by Weiss do not belong to the insect because it is *a system*, but because it is *a system with a particular kind of organization*. Clearly, if the butterfly were in a show-case it would still be a system, even in the sense of resisting change. But such a butterfly will not exhibit the phenomena demonstrated by Weiss, because it lacks a necessary part of the requisite organization. The phenomena do, as a matter of fact, present a very close analogy to the properties of certain mechanical and chemico-physical systems, but they cannot be deduced from them. Rather are they a further instance of the many ways in which

impose maximal or minimal requirements are valid only if the system is conservative or non-dissipative" (*loc. cit.*, p. 571). The present writer feels that each of these latter views may be justified. The organism may tend to maximal or minimal states, but if this is so, this seems to be due to its special biological organization of physico-chemical processes. There seems at the present stage of knowledge no reason to believe that the biological system *must* tend towards maxima or minima, unless it is isolated, when such states will be reached as the result of physico-chemical laws. But special maximal or minimal properties may be created by special organization, as is the case with the pendulum. The subject is at present highly obscure, and it seems preferable to adopt an attitude of cautious scepticism.

the organism is able to maintain its general integrity against the onslaughts of the world. They are an illustration of, not a deduction from, Weiss' statements of the general properties of systems. If anyone thinks that the results of the experiments upon butterflies could be deduced from Weiss' definition, let him explain why it is that a butterfly dies when a football is dropped on to it, while a schoolboy laughs under corresponding circumstances. An organism is a living system, organized in such a way that it possesses, to a very high degree, the power of self-conservation. Just how far and against what type of disturbance any particular organism can carry this self-conservation depends entirely on the specific organization of the organism in question, and cannot be deduced from the general fact that it is a system.[1]

Behaviour as Systemic Displacement.—In general, the systemic action of a living system is termed behaviour, a term which is properly taken to include not only overt movement of the whole body and of part relative to part, but also such action as that of the glands, the tonic activities of the muscles and the passage of the nervous impulse, none of which primarily involve externally visible motion. Thus it has been seen that

[1] An elementary inorganic example may be given. There are certain door-knockers so arranged that when the knocker is used an electric contact is made and a bell is rung. This arrangement, as an interrelated complex of elements, is clearly to be considered as a more or less isolated system, and would be so styled by both the technician and the theorist. The system is clearly, also, a mixed one. There is the mechanical partial system of the lever, the electro-chemical partial system of the batteries and circuit, etc. But the fact that the whole complex is a system does not enable us to prophesy whether or not there will be any "self-conservation" with reference to the amount of current consumed. If the knocker is badly attached, it may be that a permanent contact will be made the first time it is used, in which case current will be expended until the point of exhaustion is reached. If, however, the requisite mechanism is included, the current is automatically economized. This result is ordinarily effected by means of a spring strong enough to force the knocker back when the pressure is removed. It will be realized that the contrivance in question might be said to satisfy Weiss' criteria for a system; it is a complex that shows the tendency to remain constant as a whole in spite of a good many external changes, such as temperature, weather in general and so on. Such a power-generating system, when ideally organized, will include a mechanism for the purpose of economy; but the arrangements that fall short of the ideal are none the less to be considered systems.

the maintenance of such a homœostatic constant as that of protein involves a series of systemic actions, of which actual locomotion forms the most obvious part. The animal is "looking for food", and he looks for food because hunger has rendered him more irritable, that is to say more excitable by outside stimuli. He looks for food *until* he finds it, when the overt movement ends. But the taking of food into the mouth is only the beginning of a whole series of further processes, which end with the assimilation of part, and the rejection of part, of what has been "found", provision being made in the process for the maintenance of the homœostatic constant in question. His behaviour is rightly taken to include not only the actual movements leading to food but also the other links of the total chain, namely the preliminary changes resulting in excitability and the subsequent changes leading to digestion, assimilation and defecation. This total series represents the normal state of affairs. One part of it naturally follows upon another: we normally eat after we have exercised, and digest after we have eaten, and although it is possible artificially to eliminate one or more of the natural links in the total chain, it must always be remembered that such shortening is artificial and is apt to lead to false experimental results. We may, for instance, inject predigested food directly into the system, or spare ourselves, as we tend more and more to do as civilization advances, the preliminary food excursion by breakfast in bed. But this foreshortening of the biological chain is an artificial procedure. As such we shall later see that it leads to artificial conclusions.

When this total chain of action is considered, a fundamental difference appears between the constituent actions in that at certain stages a much greater freedom is allowed than at others. Weiss' butterflies, it will be remembered, went through three movements when placed in a darkened enclosure. They first moved up and away from the ground, then oriented themselves towards the gravitational situation and finally made a semicircular turn, ending at rest with the wings folded and the head turned towards the ground. The relative order of these movements was rigid, much more so than is the order of food reactions in most living creatures. But when the creatures were in the first phase of the total reaction, if crawling was impracticable flying in the same direction took place. As long as the end-result was attained, the intermediate series of

displacements might be totally different. This is typical of that part of the total series which involves the locomotion of the animal, and seems indeed to be typical of that part of the total series in which conditions may be variable. Thus during the food search the animal may encounter totally different sets of conditions on successive days. The details of such excursions will then be all different, the only thing in common to successive searches being the fact that food is attained at the end. The seagull may fly after a ship on one search for food, may swim on the water on another, and may settle in a field on a third: on all three occasions it may ultimately obtain nourishment. In fact, it is probable that for the beast of prey no two days' hunting are normally identical. Yet hunger is satisfied at the end of every successful day, and thirst at the end of every successful water-trip. In particular, it should be noticed that entirely different forms of locomotion may be employed to effect the same end-result. Flying, walking, swimming and diving represent entirely different neural and muscular co-ordinations among birds, yet each may be used as the medium of the series of displacements issuing in the taking of food. Though rather more striking, this diversity of locomotor methods is, however, strictly parallel to that presented by the exigencies of varying food searches in an ordinary quadruped. To chase a quarry up a hill or down a valley, to swim across a river, and so on, all require different sets of neural and muscular integrations, less obvious perhaps than the bird's change of performance, but none the less fundamentally dissimilar. Such change of mechanism is clearly advantageous if the external conditions are to be different; and according as the organism is better or worse "adapted" to such changing conditions so will the degree of flexibility be greater or less. Thus in the exigencies of the food search variation is introduced by the changeability of external conditions, while a core of invariability is provided by the constant processes at each end, for the hunger state and the processes of digestion are relatively constant. Of course it is not to be concluded that such different means of attaining the same end are completely and arbitrarily interchangeable; there must be some reason why a bird will fly across a small stretch of ground on one occasion and walk on another. And although it is not possible to lay down a definite rule covering all possible cases, yet there seems no doubt that,

in general, different forms of locomotion are called out by different organo-environmental conditions. Thus in the case of Weiss' butterflies the rule was that creeping was employed for the counter-gravitational movement. When external conditions rendered this impossible, then the flying response was used. After all, the intimate relation that exists between the external world and even the meanest living creature makes some such result inevitable. If we start with the assumption that the organism is such that it can preserve its general constancy of form and function, and that a certain deficit-state, of food or water or what not, is followed by a series of displacements the ultimate end of which is that the deficit is made good, then, whenever this deficit state occurs, if the organism is to survive, it is necessary that, whatever the topographical exigencies, there shall follow a series of movements eventuating in the appropriate end-result. In a varying world, plasticity of the response-series is necessary for survival.[1]

Such plasticity does not, then, imply any "freedom of choice" on the part of the organism. It does not mean that the butterfly could necessarily "choose" whether it was to fly or creep. The implication is rather that, granted the same general end, such as that of anti-gravitational motion, the character of the means employed for motion may differ.

Let us denote the total series of overt displacements by the term displacement series. Then we have seen that such a series, ending, for example, with the ingestion of food, must be considered as part of the general mechanism whereby organic life and limb and more specifically the homœostatic factors are kept constant. The fact of plasticity implies that *such an organic displacement series may have its final term or terms fixed, without fixing the nature or number of the preceding terms.* Such plasticity is characteristic of the purposive action already mentioned. Such displacement persists *until* the end state is reached. The end state is *implied* by the preceding states. Subjectively, it is said that the organism has the *purpose* of attaining the end, the objective counterpart of purpose thus being a plastic series of systemic displacements towards a fixed end-term. The terms purpose and plasticity really describe the same phenomenon, but stress different aspects of it.

[1] A similar plasticity has been observed by the maze experimenters in what is known as the "multiple track" experiments. See Ch. X.

The essential objective features of the phenomenon of plasticity are not peculiar to the actions of living organisms. For example, I hang a bob by a string from the ceiling, thus forming a conical pendulum. Owing to the action of gravitational and frictional agencies the pendulum will always come to rest with the string pointing to the centre of the earth. Every series of displacements will then have the same end-term, but the preceding terms of the series will be different, according to the direction and magnitude of the initial disturbance. This would have been expressed in mediæval times by saying *suum locum petit* (" it seeks its own place "), such language recognizing the essential similarity to objective purposive phenomena, exhibited in a vastly simplified form by many natural events. Many other inorganic examples could be given to show that plasticity of action directed towards a specific end-term is not peculiar to organic systems. Thus an object rolling down a slope will finally reach the lowest point possible under the given conditions. With the proper configuration the path may vary without altering the final destination. This might be done, for example, by altering the intermediate hills in the switchback railway. In just the same way, any self-regulating mechanism is so constituted that whatever the disturbance the regulating mechanism re-establishes the same state. Thus if I dampen down the revolutions of a gramophone, by putting my finger on the revolving plate, the stabilizing mechanism will re-establish the standard speed after my finger is removed. This re-establishment of a specific state will take place however much, within certain limits, the speed of revolution has been slowed, or even if the speed is accelerated by giving the disk an additional fillip. The gyroscopic compass likewise re-establishes a uniform direction whatever the deflections may be, as does the ordinary magnetic compass, again within certain limits and under certain essential conditions. It is indeed for this very end that these and other self-regulating mechanisms have been devised.

Perhaps the most striking inorganic instance is that of the " Heliotropic Machine " described by Loeb (1918):

" This ' Orientation Mechanism ' consists of a rectangular box, about three feet long, one and a half feet wide, and one foot high. This box contains all the instruments and

mechanism, and is mounted on three wheels, two of which are geared to a driving motor and the third, on the rear end, is so mounted that its bearings can be turned by solenoid electro-magnets in a horizontal plane. Two five-inch condensing lenses on the forward end appear very much like large eyes.

" If a portable electric light, such as a hand flashlight, be turned on in front of the machine it will immediately begin to move towards the light, and, moreover, will follow that light all round the room in many complex manœuvres at a speed of about three feet per second. . . . It will resume its course behind the moving light so long as the light reaches the lenses in *sufficient intensity.* Indeed, it is more faithful in this respect than the proverbial ass behind the bucket of oats. To the uninitiated the performance of the pseudo-dog is very uncanny indeed.

" . . . The principle of this orientation mechanism has been applied to the 'Hammond Dirigible Torpedo' for demonstrating what is known as *attraction by interference.* That is, if the enemy tries to interfere with the guiding station's control the torpedo will be attracted to him " (*loc. cit.*, pp. 68–9).

This astonishing mechanism, which depends for its operation on the action of light-sensitive cells coupled to electrical relays, exhibits in an excellent form the property of reaction-plasticity. Whatever the initial relative position of the light and the machine, the machine moves towards and ultimately reaches the light, provided that the light stimulus is above the threshold for the apparatus. If there are any small topographical irregularities in the way, the path will be deflected, but corresponding compensatory movements will later be made to bring the machine up to the light. It may further be noted that the machine is a partial system, the total relevant system being the light and the machine taken together. Within this system displacement takes place until certain functions have a minimal value, namely, the distance between light and apparatus. Like the living system, the machine carries its own reserves of energy.

Of course it is not here to the point to object that this and other contrivances exhibiting the property of plasticity of reaction are designed and made by human beings. Our object

here is to point out that such plasticity of action is not peculiar to living systems, not to show how living systems have acquired the property of plasticity of reaction. It is not of course denied that here, as in every other respect, the responses of the living system are far more complex than anything hitherto observed in inorganic nature.[1]

This discussion opened with a caution against claiming too much from the fact that an organism is a system. For certain specific results, there was seen to be necessary the postulate of a system with an *ad hoc* organization. This caution may here be repeated. While it is true that certain organic and inorganic systems share the plasticity property, yet in each case such property must be brought about by appropriate organization, just as the underlying thermodynamical property of striving to a maximum entropy is brought about by the fundamental organization of matter. Clearly an animal cannot exhibit plasticity of locomotor reaction such as we have been discussing unless it is provided with the necessary motor and nervous system, and so on. Further, to compare the organism in this respect with certain inorganic systems does not imply that the mechanism by which the results in question are attained is the same in both cases. Nor does such comparison dispense us from the task of studying the particular mechanisms in question; any more than does the thermodynamical demonstration that a chemical reaction must take place along certain lines dispense the chemist from the task of studying the chemical action involved. We can recognize that the "purposes", "primitive urges" and so on of the living organism correspond to characteristic systemic properties, and

[1] The term "purpose" implies certain mental states. Strictly speaking, "purposive" should bear the same implication; but this term is so convenient in the description of behaviour that it is here often used objectively, without such implication. (See note, p. 128.) I am not prepared to make a general statement as to the conditions under which such plasticity may be found in inorganic nature. There appears to be more than one class of case. The point upon which I wish to insist is that plasticity is found in connection with organic, as well as inorganic systems. That is to say, the fact of plasticity, as the term is here used, does not constitute a difference between the organic and the inorganic. I am aware that it might possibly be maintained that the *quality* of organic plasticity does constitute such a difference. Cf. Weiss on the whole question of plasticity of reaction, and also Herrick 1929), for the relation of the organic and inorganic.

still interest ourselves in the mechanism on which these properties rest.

Again, when we speak of the attainment of a specific end state as being common to both inorganic and organic systems, it must not be assumed that the end states of an inorganic and an organic system have anything more in common than the fact that they are each the constant or equilibrium state of a system reached after displacement has occurred. Thus the end state of a biological system that has been disturbed is ordinarily not the physico-chemical end state, unless the disturbance disrupts the organization of the system, that is, unless it " kills " the creature. In fact the direct function of biological organization is to ward off the attainment of physico-chemical equilibrium with the immediate environment. It is not safe to forget, when we are speaking of the equilibrium of a living creature, that we are dealing with a special biological equilibrium, made possible and superimposed, so to speak, upon the stark physics and chemistry of the situation by means of a special organization of the energy and matter involved.[1] Here, as elsewhere, biological organization results in special biological phenomena.

Restrictions.—The " primitive urges " of the organism's physical wants have been considered as the properties of a system so organized that by means of spatial displacement and otherwise certain constants are maintained to a high degree of approximation. There yet remains a further urge or rather set of urges which are not less primitive, but which have remained unconsidered, namely, those connected with sex and reproduction. The activities connected with these fundamental drives present a special problem. They are apparently not directly concerned with the preservation of bodily integrity or with a special constant necessary for such integrity. According to some workers, the sex urge is probably of central origin (Dumas, 1923, Art. " L'instinct Sexuel "). Others favour a peripheral locus (Nissen,[2] in Warden, 1931, pp. 263–322). Undoubtedly glandular factors are involved. But here our knowledge of the physiological basis of this urge

[1] Cf. Herrick, 1929, p. 45, " The working of the machine is the expression of a natural tendency to restore the equilibrium or to arrive at some new equilibrium." Herrick is here speaking of the organism as a living machine.

[2] The reference contains an excellent bibliography.

stops. Once initiated, however, the sexual reactions follow much the same course as do the food and other conservative reactions. In general, action takes place towards an external end by any means at the animal's disposal, the same end being attained by systemic displacement series differing widely according to the circumstances encountered. Internal changes seem to take place which sensitize the organism, at least the higher female organism, to certain stimuli, which thus become effective to disturb the general mechanical equilibrium of the system with reference to its environment and thus initiate movement. There is a parallel in the case of hunger, which produces greater activity on the part of the organism, sensitizing it to " food " and other stimuli (Carlson, 1916), though the nature of the internal change is at present unknown in the case of sex.

Nor can we describe the specifically reproductive activities as yet in terms of systemic reaction. Recent researches in embryology seem to show that the growth of the embryo consists in the development of a total system, a pattern, which is a whole from the time of fertilization onwards. Upon this whole pattern appear in orderly succession details which are at all times apparently subordinated to the whole structure, thus forming a more and more elaborate scheme which is at any instant fully integrated, making biological unity within a unity. This lesser unity is not, however, a partial system of the ordinary kind, for its final upshot is a completely separate system, the relationship between embryo and the mother in the mammals thus being of a unique kind. Theory does not at present suffice to deal more adequately with this fundamental phenomenon.[1]

Summary.—We thus have a picture of the organism as a physical system of prodigious complexity, many striking properties of which belong to it as a specifically organized system, rather than to the working of any unique vital processes. The specific way in which these systemic properties show themselves depends upon the peculiar organization of the system in question, just as the specific manner in which

[1] See Coghill (1929); Tracy, H. C. (1926). A discussion of the tl of embryonic development will be found in Bertalanffy (1928). For a discussion of other possible " drives " see Richter (1927), who gives an interesting account of nest-building as part of the temperature conserving mechanism.

the laws of physical science operate in any given case depends upon the peculiar nature of the inorganic system in question. Special mechanisms enable the organism after disturbance to come to a conservative equilibrium, that is, one which is nearer to the original equilibrium pattern than would be the case without such mechanisms. Thus it tends to preserve itself. This power of self-maintenance, effected by means of such activities as food taking and breathing, has a simple counterpart in the self-conservative tendency exhibited by certain inorganic systems. The organism's power of economical response again depends on the establishment of a special biological equilibrium to change of external conditions. The power of reproduction is not here considered.

CHAPTER V

THE FIRST APPROACH TO THE GENERAL PROBLEM OF LEARNING

"The stone which by nature moves downwards cannot be habituated to move upwards, not even if one tries to train it by throwing it up ten thousand times." (Aristotle's *Ethics*.)

Learning as an Extension of Organic Response.—Consider any of the higher organisms in the ceaseless turmoil and flux that is the environment. At every moment external changes are occurring which disturb the organic equilibrium. The varying conditions of temperature, of atmospheric pressure, and humidity; the protean pattern of the sound waves, a pattern now of an astonishing and powerful complexity, now dwindling to a delicate rhythm or confused monotony, perhaps never entirely absent; the ceaselessly tumbling kaleidoscope of the etheric vibrations; the highly diversified changes of atmospherically born chemicals stimulating the organs of smell; these in addition to the more intimate and equally varying nexus of the environment-in-contact, all call out from the organism at every moment appropriate response with a corresponding expenditure of energy. Yet, astonishing though the picture is of so delicately responsive a self-maintaining mechanism, it does not tell half the story.

For if one attentively regards the astonishing drama of any higher creature's life, two further principles seem to thrust themselves from out of the total flurry of organic action and reaction. The circumstances of the present apparently affect the animal in relation to his past; and equally they seem also to affect him in relation to his future. "The lion roaring after his prey, seeking whom he may devour" apparently determines what he does by the fact that he is out for food. Equally true it is that he apparently determines what he does by his experience of previous chases; he knows, the naturalist tells us, where to find what he wants. He would

usually be said to go out with the purpose of finding food, and to have learned where to find it; his actions, that is to say, are most adequately described not as discrete, independent, but in relation to other actions with which they naturally form a group. The higher animals seem not only to act and to act with a purpose, but to learn. Now it has been seen that the displacement of an inorganic system towards a maximal or minimal state may provide an inorganic analogy to purposive action; the concept of final cause was claimed, for example, by no less a thinker than Euler to be applicable to the events described by physical science.[1] Learning, however, as a mechanism of utilizing past experience for the organism's preservation, seems surely to be a peculiar prerogative of Life, admitting of no inorganic analogy. Inorganic systems, again, may perhaps be thought of as responding in a simple way to external change, if they are so organized that external disturbance is followed by compensatory systemic displacement. But the power of learning seems uniquely different not only from inorganic action but also from other organic response.

Learning not Unique.—Yet this apparent uniqueness is illusory. It comes from the fact that we are prone to consider so-called learned actions in isolation instead of in their proper context as forming part of a series. A learned action should be considered strictly in relation to all those preceding actions which cause us to say that it is learned. If it is so considered it loses its quality of uniqueness among organic phenomena, and is seen to be merely a more elaborate response of the type already considered. A cat has learned to go to its plate for food when it hears the clatter of dishes. If we consider the cat's performance on any one day, the animal does seem to have accomplished a uniquely "rational" feat, in that it makes a specific and new adjustment to its environment today because it has had certain experiences yesterday, the day before, and the day before that. If, however, these actions which were performed while the animal was learning the action are grouped, as they belong, together, to form the total series of actions performed while the animal was learning the performance, then we have a series of related runs at food-time finally culminating in

[1] "There can be no possible doubt that all natural phenomena may be determined from final causes by the method of maxima and minima, as well as from efficient causes." Quoted by Gengerelli, 1930, p. 231.

a direct run to the plate in the kitchen. That is to say, the animal has made a gradual adjustment to the food situation, an adjustment which is only relatively complete when the action is fully " learned ". Such a process will, it is true, differ in many respects from the more elementary adjustments already considered. Consider three cases : first suppose a cat to respond to a noise by pricking the ears and turning her head ; suppose her secondly to respond to the smell of food by jumping off a chair and running to the saucer ; finally suppose her to have " learned " to run for food when she hears the central heating furnace being stoked for the night. In each of these cases we have the organism confronted with a change of environmental conditions to which adjustment is made. The changing element is generally called the situation or stimulus. When the animal turns her head or leaps from the chair the stimulus is the new auditory or olfactory element. When she has learned to run to food the new set of environmental conditions includes the total series of situations in which the animal finds herself at the evening feeding time for a week. Just as the animal makes an adjustment to the situation including the olfactory and auditory stimuli, so it makes an adjustment to the total serial food situation for a week. It runs each day, but its path differs from day to day and finally leads directly to food. In the first case " response " involves a relatively simple succession of muscular actions, with movement of only part of the body ; in the second case, where the animal jumps from a chair, there is a succession of movements and postures involving locomotion of the entire body ; while " learning " involves a unitary series of such total locomotor activities. That is to say, the learned activity here involves a series each term of which is itself a succession of systemic displacements ; a series each term of which is a series of responses. This can be generalized : *Learning involves the process of making a systemic adjustment, which may be expressed in the form of a series each term of which is a series of more elementary adjustments. It seems to be unique because there is a tendency to analyse the actions of the organism into single responses without reference to the series which is their context* (cf. Coghill, 1929, p. 90).

In order to make this clear another example will be presented. When an individual learns to typewrite, he is confronted with the situation presented by the typewriter and a general social

situation. This total situation the skilled typist meets by certain motions of the fingers and arms, these motions being specific to certain visual stimuli—the transcript or stenographic notes—forming at each performance a fresh part of the general environment. Learning to typewrite has taken place, let us say, when the individual confronted with a manuscript as part of a general social setting can perform in a certain minimal time and with a certain minimum of errors the specific movements necessary to make a " copy ". This response is different at the end of the learning period from what it was at the beginning, and if the learning curves are examined they show a progressively larger number of strokes per second and a progressively fewer number of " mistakes ". When the learner has attained, let us say, nine strokes per second and 99·8 per cent of accuracy he has completed the learning stage. Ordinarily we say that he has attained this skill " because of his lessons in typing ", or that he is able to make a certain adjustment to the situation " because of his past experience ". This clearly makes the activity of learning unique, a function " *sui generis* ".

Sometimes, indeed, it is said that he can type better today because of his " Memory " of his lessons ; or it is a " mnemic accumulation " or an " engram " that enables him to type better. Such special terms of course only push the mystery a little farther back. They are as harmful as was the mediæval explanation that the water rose in a pump because nature abhors a vacuum, and are exactly parallel to the " first principles ", against which Vicq d'Azyr thought it necessary to warn his readers.[1] Such explanations are not necessary if instead of considering the finished product of learning, namely, the letter typed at nine strokes per second, the total series of relevant situations is considered together with the total series of responses. Then it is seen what has happened. The human organism has been confronted, on a number of successive occasions, with situations presenting a certain similarity, and has made a total unified adjustment to the total series of these situations, the final stage being the typing of a letter at a certain speed and accuracy. To be sure, this highly complex example does not provide a series of identical repeated " learning " situations. A typist must be able to copy " new " as well as familiar manuscript. When he has

[1] See p. 1.

learned to do this he ultimately makes an expert performance which is part of a total Response to the *total series* of learning situations, including the "new" copy, and for this he must select and re-integrate. The astonishing power of the human nervous system is seen not least in the ability to attain by a complex integrative process a constant state of reaction to a total set of situations in which the similar elements are distributed in such a complicated manner. As will be seen later, similarity and integration there must be for "learning".

This is of course no new principle of explanation. It has said nothing as to how the result has been effected. Quite the contrary. It has left new principles of explanation to the makers of such terms as "mnemic accumulation", claiming rather that this instance of learning is to be considered as an extension of the organism's power of adjusting to a single stimulus or situation. While the full implication of the statement cannot be made clear at this stage of the discussion, yet it may be repeated that starting from the conception of the organism as reacting or adjusting to a single stimulus or situation, learning must be considered as the process of making an adjustment or total reaction to a total series of such stimuli or situations. It is essentially similar to the process familiar as the act of responding to a situation by means of an integrative process. It involves no new principle, but merely the extension of one already known. The remainder of the chapter will be devoted mainly to an exposition of this central idea; certain immediately occurring objections will also be met.[1]

Delimitation of the Term Learning.—The fact that there is no sharp line of division between the more elementary response and the "learned" activity is well brought out by analysis

[1] It is interesting to find that the term "learning" has only recently found its way into psychological discussion. Thus no specific mention of it is to be found in the Index of Ward (1918), Stout (1896), or Külpe (1895). There is no heading "learning" in Baldwin's *Dictionary of Philosophy*, nor in the Index of James' treatise (1890). For Wundt, learning is practically synonymous with the memorization of words and by syllables, although reference is made to the animal's "capacity to learn" (1903, Index). This relative silence does not of course mean that the phenomena were not studied, but rather that they were given different names, often such as to imply a special mechanism. The terms "Memory", habit, training of Attention, Apperception, were thus used.

of the term "learning" as it is commonly used (p. 5). Not every series of successive actions performed by an organism is considered to exhibit learning. There must be, as we have seen, modification of later terms of the series because of the fact that earlier ones have occurred, and indeed it is sometimes assumed that modification, at least under relatively complex conditions, is synonymous with learning. But this is not so. Where there is learning the later actions of the series ordinarily differ from the earlier ones in the direction of the organism's advantage. In the terms of the last chapter, it may be said that the term "learning" is not ordinarily employed unless the modification of action has furthered the conservation of the system. A gull, let us say, has learned to fly after the ships as they leave the harbour. Clearly the learning rests on the fact that the bird is able to obtain bigger and better meals, and easier ones. That is to say, its energy intake is more economically effected. If, after finding food behind a number of ships, it had so modified its reactions that it flew in the opposite direction whenever a ship appeared, we should hardly say that learning had taken place. A burnt child that used its experience in order to acquire deftness in plunging its hand into the flames would be a candidate for an institution. In order then to exhibit learning, a series of organic actions must first of all be such that later terms presuppose earlier ones, which is the same thing as to say that modification has taken place; but in addition the series will generally show a general approximation to an optimal term, optimal, that is to say, from the point of view of systemic conservation. Because the series consists of related terms it is a unity; [1] and performance must converge to an optimum which is fixed physiologically or psychologically. Improvement cannot go on for ever.

Types of Learning Series.—Now most of the series that are actually observed fall naturally into three or four main divisions, according to the types of stimuli and responses involved. These divisions correspond ultimately to a classification of action series in general, but the discussion of the general problem offers complexities which are not germane to our present purpose. Only such cases will therefore be

[1] "Logically and really all relations imply a whole to which the terms contribute and by which the terms are qualified" (Bradley, 1930, p. 521).

discussed as appear to offer possibilities for learning or at least of a process allied to it.

Suppose first of all that a series of successive stimuli affects one receptor field only, and is followed by a series of similar responses of the same effector or effector group. Consider, that is to say, the successive sets of environmental conditions constant save for periodic change in those affecting a single receptor field. Such a simple series of responses to simple stimulation may be represented as follows:

$$S_1R_1 \quad S_2R_2 \quad S_3R_3 \quad . \quad . \quad S_nR_n \text{ (type A).}$$

This formula represents a very common type of reaction to repeated stimulation. A particularly common case is where action gradually diminishes to the zero point. For example, a turtle is tapped with a pencil at successive intervals of two seconds. The reaction, which consists in the withdrawal of a foot, gradually decreases until the zero point. Here is a quantitative change of visible response, the same effectors being involved in each reaction, but to a progressively less and less extent. The experiment clearly shows modification of action because of previous action. A limit is reached, namely, that of zero motion, although it might be thought that in this simple example the term "optimum" would perhaps be out of place. However, zero motion is clearly an optimum as regards economy of energy-expenditure, so that there is some justification for the use of the term.[1] In many cases reaction to a simple repeated stimulus does not thus diminish, and here at least the term learning would be out of place. For example, a turtle was tapped at intervals not of two but of one second; reaction progressively increased and the whole animal gradually became more and more "excited", until every limb was violently moving, with apparently a qualitatively different systematization from that observed at first. The "swimming" movements seem to set in, which appear to involve at least a different organization of the same muscles. Here, under the conditions of the experiment, "learning" has clearly not taken place. A physiological limit is ultimately reached, but this can hardly be called an optimum, for *under the given conditions* this limiting reaction does not differ from earlier ones in the direction of energy economy, or systemic conservation. The

[1] See Gengerelli, 1930.

same result was observed by Jennings on continued stimulation of the earthworm (1906). But there are further cases where a repeated stimulus is followed by a reaction that diminishes and disappears, but where nevertheless it would certainly be a misnomer to use the term " learning ". Thus we may have a series of actions conforming to the type given, but without convergence to a real optimal term. For example, if a stimulus is repeatedly used to elicit a " reflex " from such an animal as a dog, reaction will persist for a long time and will ultimately fade away from " fatigue ". If such fatigue is carried to the point of physiological exhaustion and death, the final state can hardly be said to be an " optimum ". If, however, reaction ceases before the state of absolute exhaustion or depletion of physiological reserves, then we have again a borderline case. In this simple mode, indeed, we seem to be in general on the borderline of learning. Generally speaking, it is hard to draw the line at any one place and say, " Here learning begins." Between " fatigue ", which is primarily a protective mechanism, and learning, it is often hard to make a distinction.

This discussion of the simplest type of reaction to a repeated stimulus illustrates a statement which is true of the more complicated cases also. Learning, or a process allied to it, may take place in any of three or four modes which will be considered, but not all action-series fitting into these modes give instances of learning. The types give no criterion of learning, but they will be found useful in showing the relation of the more complex to the simpler cases. The general possibilities for " learning " in the mode represented by this type of series will be considered later in the chapter. It will be observed that here, as in all the other types to be considered, each term of the series involves a relatively complete response, that is to say a re-establishment of equilibrium to a change of external conditions, the whole series representing, therefore, a total adjustment that is a complex of minor adjustments. Learning, if it occurs in this mode, is therefore constituted in a number of related terms, each of which is a relatively complete stimulus-response element. This is characteristic of learning as it is found in the other modes also, though an apparent exception will be discussed later.

The first type, which will hereafter be called type A, may be complicated by changing the quality of the response at

one or more points, keeping the stimulus unchanged throughout the series. The resulting action-series may thus be represented:

$$S_1R_1 \quad S_2R_2 \quad S_3R_3 \quad . \quad . \quad S_{p_1}R_{p_1} \quad S_{p_2}R_{p_2}$$ [1] (type B).

Here as before there is a succession of similar stimuli, followed by qualitatively the same response up to the point S_pR_p, at which, however, a fresh quality of response occurs.

Such a fresh quality of response may be brought about by the use of different effectors, as in the case of flying or walking in birds, or possibly by a different systematization of the same effectors, as perhaps occurred in the turtle experiment to which reference has been made. It is important to observe that such change of response may mean a qualitative difference as regards the effectors, while the general nature of the total response remains unchanged. Thus the writer observed a rook to take a few steps towards the rest of the flock, and cover the remaining distance by flying. Here, indeed, an entirely different set of effectors is put into operation, but it is fair to say that the general character of the response apparently remained unaltered. Movement was still *towards* the flock. The tactics, if one may say so, have changed, but the strategy remains the same. On the other hand, however, even though the stimulus is kept strictly constant, the whole nature of the response may apparently change at a definite point. This is excellently illustrated by Jennings' experiments on the sea anemone. This observer found that if these animals were stimulated by the dropping of water, at first the tentacles were withdrawn, then this reponse disappeared, thus giving an example of the type A series with the reaction diminishing to zero. If, however, the drops were continued the animal began to shrink away. Here seems to be a reaction of a slightly different quality from the mere withdrawal of the tentacles. In the same way, an anemone was stimulated fifteen times by light strokes of a rod. At the first stroke it contracted strongly, and continued the reaction fifteen times, when it bent over to one side and extended in a new direction. An anemone may even release its foothold and move to a new position. " Thus to the same stimulus when repeated many times the anemone reacts first by contraction, then by turning repeatedly

[1] Here, as in the other formulæ, there is assumed a constant stimulus background. According to the general convention, S signifies the changing element on this background.

into new positions, then by moving away " (Jennings, 1905). Similar phenomena have been observed by the same investigator on the unicellular organism, Stentor, and on Hydra by Wagner (Jennings, 1902; Wagner, 1904). Another type of qualitative change of reaction to continued stimulation is to be found in cases where there are two conflicting mechanisms involved. This is illustrated by the " curiosity " reaction of many animals, which alternates between flight and attraction, though here the stimulus is generally continuous. It seems that hardly enough attention has been given to the possibility of learning in this mode. The complication which it represents may occur in any of the more complicated types of action-series to be discussed later. To anticipate, there are instances recorded where a rat, having learned a simple maze, jumped over one of the walls, thus short circuiting the obstruction to the reward. Here the learning process involved a reaction of an entirely different quality from those preceding it, for of course the action of a rat in jumping uses a systematization of the motor system different from that employed in running. A similar result was reached in a well-known experiment of Lashley who performed certain operations on a rat that had learned to pass down the brighter of two passages for food. After the operation the rat could not turn to the left; when such a turn was made necessary by the apparatus the rat turned through three right angles to the right. This is, of course, not strictly an instance of learning with a qualitative change of response, but it offers a close parallel. Lashley has obtained similar results with other rats after cerebral operation (Lashley, 1924; cf. 1929 and 1930). It is highly desirable that there should be made experiments with amphibians, birds and other organisms with more than one means of locomotion at their disposal. Although prophecy is dangerous, it seems probable that such experiments would disclose many cases where learning involved such a qualitative change of response, though whether this would involve any more than what has been called a change in tactics is more doubtful. Such experiments would corroborate the theory of the Gestalt school that on the conscious side learning involves insight, and indeed some of the experimental results adduced by this school would seem to fit into the rubric of this mode.

This type of action-series with a qualitative change of

response to a succession of similar stimuli is a variant of the original type, which exhibits similar responses under the same conditions. Complication of a different form may be introduced by adding to the simple stimulus terms a repeated stimulus affecting a second receptor field. In this case, instead of a single periodic change in the environment, we now have two such changes occurring at closely recurring intervals of time. The resulting series may be represented as follows :

$$\frac{S_1}{T_1} - R_1 \quad \frac{S_2}{T_2} - R_2 \quad \frac{S_3}{T_3} - R_3 \quad . \quad . \quad \frac{S_n}{T_n} - R_n$$

(type C)

where S_1, S_2, etc., and T_1, T_2, etc., are stimuli affecting different receptor fields, R_1R_2 the *total* response, both S_1S_2, etc., and T_1T_2, etc., being repeated situations or stimuli. For example, food and light are presented simultaneously to a dog on a number of successive occasions. Here successive presentations of food are represented by the S terms, and successive light stimulations by the T terms. This is, of course, the formula of the well-known conditioned reflex experiments, in which, however, an additional T term is introduced at the end of the series without the S. The light is shown without the food, and if " learning " has taken place the food response follows. The relation of such learning to the organism's advantage will be discussed later.

It will be seen that type C is a simple development from the primary type, being built up from it by the addition of a stimulus affecting another sense organ. It presupposes, of course, the existence of at least two kinds of sense organ, while series of types A and B may theoretically be, and in fact are, found where there are no clearly differentiated receptors at all. Type C then presupposes an organism of a considerably higher type than either of those preceding, not only because such an organism must possess at least two kinds of receptor but also because it must possess a nervous system adequate to effect the necessary synthesis of receptor-data. It is further to be noted that the internal receptors of the body, the proprio-ceptors and possibly the entero-ceptors also, may conceivably form the second external receptor field. As a matter of fact Kinæsthesis, the stimulation of the proprioceptors, generally acts as a complicating or third

receptor field, at least in experiments on motor learning, where it has been found to play an important part. Now there is no readily occurring theoretical reason why "learning" of the "associative" type should not occur between two main receptor fields, one of which is external and the other kinæsthetic. Such experiments as those of Miss Swartz (1929) where worms were trained to turn in one direction rather than another by the use of an electric shock perhaps provide an approximation to such conditions,[1] although it is difficult to know how far tactile stimuli are involved in this type of experiment. In general, however, even the simplest action of any of the higher animals is accompanied by an extraordinarily complex nexus of internal and external stimulation, and indeed the conditioned reflex experiment, which Pavlov considered to be "the simplest possible case", has been shown by Beritoff to be a highly complicated affair[2] (1924). The formula by which the elementary type of association has been represented thus depicts an ideal which can never, perhaps, be actually realized; in which respect it resembles all other formulæ.

Learning in the mode of type C, the simple associatory combination, may be further complicated by increasing the complexity of one of the stimuli. This has been done, for example, in the puzzle-box and maze experiments. By this means complexity of response is increased. The animal, on first being placed in the maze, performs a very complicated series of movements which ceases when the reward is obtained. This series gradually becomes less and less complex until it attains the utmost limit of simplicity compatible with the given conditions. But though such experiments present an apparently different problem from that of the simpler conditioned response, the two are of an essentially similar nature. In each there are two fundamentally contrasting sets of stimuli, the "reward" and the "non-reward". In the two types of experiment these are food and maze, food and light respectively. In each a necessary part of the process of learning consists in associating these two different stimuli, in providing, in other words, a "motive" for the maze running

[1] Yerkes' (1912) well-known experiments involved other forms of stimulation.

[2] Beritoff worked with the motor reflex, while Pavlov's original experiments of course used a glandular response. Brain, 47, p. 358.

or salivation as the case may be. It may be said that in each, also, it will appear later that " improvement " of performance takes place. The difference between these two highly standardized methods is that the maze experiment offers a process of much greater complexity, where the animal has its natural liberty of locomotion, a point which will later be seen to be of great importance. The maze experiments correspond, of course, much more closely to the actual conditions of the animal's everyday life, and for that reason it might seem safer to begin in our analysis from the complex end. While it is indeed often preferable to proceed from the complex to the simple, rather than vice versa, yet the method actually adopted has the advantage that it gives at least a logical classification of the cases of learning actually observed. But whether the analysis be begun from the complex or the simple end, the same general point is illustrated. *The experimental instances where modification of response would ordinarily be said to have occurred may be arranged in a progressive series ; at one end stand the most complex cases such as the mazes and puzzle-box experiments and incidentally the chimpanzee experiments of Professor Köhler ; at the other the simplest cases of modification hitherto observed, namely what is ordinarily termed negative adaptation.* Between the two and representing an intermediate stage in complexity both of stimulus and reaction falls the conditioned response. Further, no strict line of demarcation appears between the simpler forms of modification and the ordinary single response. For, as we have seen, we should not focus attention separately upon the individual actions forming a series in which modification has taken place ; these actions should rather be considered in relation to each other and as forming a totality which may be considered as a total Response or adjustment. Such total responses to a complex stimulus-pattern differ in complexity, not in quality, from the simpler response to a single stimulus which is typically a systemic displacement leading to a fresh conservative equilibrium.

We thus have a picture of Response growing gradually more and more complicated, beginning with the primitive reaction characteristic of the unicellulars, gradually increasing in complexity to become reflex action, and diverging, perhaps, at one point to follow the track which will later become " instinct ". This is apparently, if and where it exists, a

unitary or " closed " response to a highly involved situation-complex which is of a serial nature but which typically contains no repeated elements and thus no possibility of " improvement ". In its purest form such " instinctive " action should appear as a fully-fledged and complete act of self-preservation, reproduction or what not, on the first occasion that the total stimulus-complex is presented. It is more complex than a reflex both because of the greater complexity of the stimulus that calls it forth and because of the greater complexity of the response. It is, as Koffka and others have pointed out, more than the arithmetical sum of a number of reflexes because each individual action has meaning only in relation to the whole. That is to say, it is a totality, a unit-series of actions. How far the actions of the lower organisms conform to this pattern it is difficult to say. We know much less for certain about Instinct than we did thirty years ago.[1] But at least the conception is useful as showing one possible line along which organic response may have evolved, and there is a strong probability that at least an approximation to this pattern of behaviour exists in many forms of life.

In learning we have a departure of a different kind. Here is also a development in the direction of greater complexity, but the complexity is of a different order. In place of a combination of serial responses into one Response, which appears completely integrated at the first presentation of the necessary serial stimulus, we have a total response to a very special kind of Situation-complex, namely one that is made up of temporally disconnected similar elements. Or rather, we may say that objectively the situation calling forth the response is temporally discrete, but in relation to the organism —in the terms of introspective psychology, subjectively—it is connected, unified into a single situation. We speak, indeed, of a person or an animal " connecting " what happened yesterday with what happens today, and such linking up is considered the mark of a higher organism. A mouse has " learned " to run a simple maze by traversing it, let us say, fifteen times. From the point of view of physical science the successive stimulus-complexes formed by the thrice-daily presentations of the maze are discrete in time. As stimuli-

[1] See Bernard (1924) for an analysis of the general conception. Interesting examples of criticism on detailed points are to be found in Wheeler (1929).

in-relation-to-the-mouse they must be considered in connection with each other; and unless there occurs in the animal some kind of synthesis to which they are related, learning cannot take place.[1] As a matter of actual fact those forms of life that have progressed along this line of advance have apparently found themselves ultimately able to make adequate response to situations of a greater degree of complexity than those which have apparently progressed farthest along the other, "instinctive", line, namely the insects. Whether this is an inevitable consequence of the two forms of response, or whether it is an accident, we are not at present in the position to say; [2] nor is it germane to our purpose to enquire. Thus we are enabled to trace the evolutionary genesis of learning. Beginning with the simplest reaction of a primitive creature, there is gradually developed by the acquisition of the requisite organic structures the power to adjust to more and more complex situations. This development may take place along several lines; it may consist in the possibility of a more and more efficient response to a more and more complex and delicate nexus of simultaneous stimuli, such development being seen by contrasting the unicellulars with the simpler fishes, with their much superior mechanism both of receptors integration and of response; or there may be developed the ability to make a unified response to a more elaborate unit-block of stimuli in more or less continuous succession, as is presumably the case where instinctive activity exists; or, lastly, there may come the ability to make a unified total response to a still more complex situation, consisting of temporally discrete but "similar" stimuli; these stimuli may affect one receptor field only, or more than one, and each of them will call for a response which can be considered as more or less complete in itself but whose full meaning can only be seen in relation to the whole response to the total situation. Response to such a situation, comprising temporally discrete but similar stimuli, is, at least in the more complex cases, called learning.

The question naturally occurs, is there not a qualitative difference at the point of transition from type A to type C,

[1] "Conscious" connection is, of course, not implied.

[2] It will be realized that there is no intention of maintaining that either "instinct" or "learning" belongs exclusively to any form of life.

where response of the type of negative adaptation goes over into association? This was the opinion of Loeb (1900), for example, who maintained that at certain points in the animal scale there appears an entirely new phenomenon, psychic memory, which is the mark that psychic processes are present. Similarly S. J. Holmes contrasts adaptation as non-intelligent behaviour with the formation of habit, which he says is based on the making of associations (1911, p. 139).[1] Piéron, on the other hand, maintains that the difference is mainly one of complexity (1920, p. 143). There can be no doubt that he is right. For consider the evolutionary prototype of the higher organisms, namely, the unicellular animal. It consists of relatively undifferentiated protoplasm in which reception and response are effected by a single cell. As an organism, however, that is a living system, it functions as a unity.[2] Whatever action a protozoan makes, whether it is "chasing" food or moving away from an intense light, it makes the action as a whole. It is not the case that one-half of the animal moves towards the food, the other half away from it, or that one part moves towards the light, another at an angle to it. Further, the possession of receptor cells in higher organisms brings no novelty as regards excitability. The evolutionary prototype of the cells in the receptor is still the generally excitable protoplasm, but there has taken place an accentuation of the power and delicacy of excitability by certain kinds of stimuli. "The main function of the receptor is to lower the threshold of excitability of the arc for one kind

[1] With this reference should be compared Buytendijk (1919*a*), containing a review of an article written in Dutch where he claims that the unicellulars acquire no habits, the molluscs can acquire a quantitative change in an instinct, not a qualitative one, while the crustaceans can acquire a habit. Compare also Smith (1908) on "The Limits of Educability in Paramœcium," where it is apparently implied that "associative memory" marks a distinct stage. In the absence of specialized receptors it is difficult to see how such memory would be possible. The view of Buytendijk is a little confusing. He apparently agrees with Jennings that the unicellulars present habits of short duration (1919), but seems to state in his book that the unicellulars do not exhibit habits, thus apparently regarding the time difference as effecting a qualitative change. Buytendijk (1919) has apparently disproved the contention of Smith (1908) and of Day and Bentley (1911) that paramœcium can learn.

[2] Cf. *e.g.* Jennings (1904) the paper on tropisms, where it is insisted that the unicellular organism behaves as a unity.

of stimulus and to heighten it for all others" (Sherrington, 1906, p. 11). When two receptor organs have developed we have then merely a specialization in two sets of cells of excitable properties possessed in the lowest forms by all the cell indiscriminately. When such differentiation into two specific receptor organs has taken place the organism must still function as a unity. It must still be able to offer integrated action to changes occurring in the total environmental complex, whether these changes are such as correspond specifically to the newly acquired receptors or not. The only novelty conferred by the receptors is the possibility of more finely discriminated and more delicate reaction to the two kinds of environmental changes in question. Of course, parallel with the development of the receptors there goes generally a more efficient mechanism of transmission and of action; the organism acquires a nervous system, let us say, and a motor system. Yet even these are but developments of properties originally possessed by the primitive undifferentiated protoplasm. Their function is to maintain and improve an already existing synthetic power of action, not to create a new power. "The initial reaction or *reception* of the stimulus, the spatial transmission or *conduction* of the reaction, and the motor or other *end-effect*, are all processes that occur in one and the same living structure [amœba]" (*ibid.*, p. 6). Clearly, then, what will later in the evolutionary scale be known as "associative memory", the integration of, and unified reaction to, the stimuli affecting two receptors cannot be regarded as a novelty. The burden of proof is certainly with those who claim that, when concentrating the process of stimulation along two or more channels, the organism must at the same time develop a qualitatively new mechanism of "association" in order to effect an integration that has been present from the beginning.

It may here be objected that two meanings of the term Association are confused. When two simultaneous stimuli or situation-elements encounter the organism an integrated or associated response must indeed be made to them. But this integration, it may be claimed, is not the same thing as association, whereby at a later period of time one situation-element is found to have taken upon itself the response originally attaching to the other only, giving the power of "psychic" insight into the future.

This, of course, assumes the point at issue. There is on examination no reason for believing that there is a qualitative difference between integration whereby a unified response is made as the result of simultaneously excited receptors and that by which a unified (learned) response is made as the result of successively excited receptors. All integration is four dimensional, it is integration of organic processes, events, corresponding to external changes that are four dimensional also. "Space and time spring from a common root—the ultimate fact of experience is a space-time fact" (Whitehead, 1920, p. 132). In certain cases we are able, by a process of abstraction, to consider the external events as simultaneous and as relatively timeless; while in others the time co-ordinate is too obtrusive to allow this. Time must be regarded as an abstraction from events; prominence of the time factor cannot be considered as conferring a unique quality on certain integrative processes. "In the structure of (the) space-time manifold space is not finally discriminated from time" (Whitehead, *ibid.*, p. 173). When used objectively such words as "memory" have only been made necessary because of an ineradicably old-fashioned habit of thought, resting on the illegitimate abstraction of time from events. If, then, we regard the act of "associative memory" not in itself but together with the preceding acts that go to form the total relevant series, we have a unified total Response to a four-dimensional Situation, requiring four-dimensional integration of neural impulses. Such four-dimensional synthesis we have seen to take place in every act of integration; it is only by an abstraction that a purely spatial integration seems possible. Once, then, granting the essential similarity of the underlying process of unification in what is called "integration" and "association", the uniqueness of "association" disappears.

For example, at any stage of the learning process all the acts leading to the formation of a conditioned reflex are related to form a unity, while the successive presentations of the corresponding situation, the bell-food complex, must likewise be considered as forming a single total Situation. So that at any presentation after the first the sound of the bell must be considered not in isolation but as part of a total situation including also past presentations of bell and food. Because the bell is part of this total situation, reaction to it is modified accordingly, just as reaction to any stimulus may be modified

by the total situational context. This is, of course, what integration implies, that a response is made to the total situation, so that the effect of any single element of the situation may thus be different according to the nature of the total context of which it forms part. Now in the case of the bell-food complex, the modification of bell-response which takes place because the bell has been included in a new temporally extended context is called the process of forming the conditioned response. Objectively speaking, we have here no mystical prevision of the future, but rather reaction to an element which is part of a total Situation with its roots in the past. It is often said that the bell serves as the signal for food to come in the future. It is more correct to say that the bell has become part of a total situation extending into the past.

To take another example: A cat responds to a situation, in which a chair and milk on the shelf form part, by jumping on the chair and thence to the shelf. A unitary, integrated response has been made to this complex situation. The same animal encounters a ringing bell together with a saucer of milk on one day, the same combination on the next, and on the third day hears the ringing bell in another part of the house. The animal "connects" all these events together—again we are not implying a "conscious" connection—and runs to the saucer. The unified response on the third occasion is part of a larger unity comprising behaviour on three days. In view of the modern doctrine of space and time there can be no reason for postulating that such "connection" is somehow different from the central unifying process which takes place when the cat jumps up for the milk. There is, of course, implied no explanation of the mechanism employed in the two cases. Our concern here is merely to make clear the fact that there is no reason for assuming qualitatively different mechanisms to be operative in the two instances. It is remarkable that the cat can run to the saucer from another part of the house; it is equally remarkable that it can jump on a chair to get milk. The one process is no more astonishing than the other in spite of the fact that the special name "learning" has been given to it.

It may be objected that at the beginning of the learning period the integration is not complete, and that it is the peculiar function of the learning process to effect a gradual fusion of nervous impulses originating in the two receptors

concerned. Thus the conditioned response is but gradually developed in the dog. At first the impulses from the auditory and the taste receptors are received, but are unintegrated; by means of a number of repetitions of the combination neural fusion takes place of the auditory and gustatory neural pattern. This gradual process, it might be said, is peculiarly learning. Such a statement, however, tends to the fault of which complaint has already been made. At its best it is dangerous. It tends to consider the neural responses to successive presentations of the combined stimulus as separate, and thus finds it necessary to invoke a special faculty of " learning " to account for the gradual neural fusion which takes place as the result of a number of separate presentations. If the successive presentations are regarded as aspects of one total Situation, then the gradual fusion of neural impulses may be regarded as part of the general and total adjustment to this Situation, without the necessity of postulating a special faculty. The argument really reduces to this : if the successive presentations necessary for the learning process are regarded as discrete, disconnected stimuli, then it is apparently necessary to postulate a faculty *sui generis* to account for " modification by experience ". If, however, they are considered as aspects of the same, more inclusive, total Situation, then the " learning process " appears as an extension of the familiar activity of response, and no new faculty is necessary. If the principle of parsimony is adopted, we thus have an argument in favour of the present theory.

At first sight it may seem impossible to treat " entirely separate " situations as parts of one larger whole. But this is exactly what we do with spatially extended situations, which are considered to comprise spatially separated smaller elements forming one inclusive larger pattern. In view again of the present attitude of physical science to the space-time continuum, the burden of proof certainly lies with those who claim that we may consider an organism as reacting to a single situation of spatially separated elements, but not to a single situation comprising temporally separated elements. Köhler's chimpanzee connected into a unity two sticks and the banana which were simultaneously presented in space. The animal's reaction is to the total spatio-temporal situation including these three specific elements, which the creative, integrative activity of the animal has interrelated. In the

same way the animal learning by repetition effects a similar creative integration involving the similar elements in the repeated situation; he connects what happens today with what happened yesterday, the difference being that here the "temporal", there the "spatial" aspect is more obvious.

It may here be repeated that between the activities usually considered to be due to a special process of learning and those generally considered as simple responses there is a further parallel in that each class of activity is typically conservative of the living system concerned. Professor Haldane's dictum may be repeated from the last chapter. "Biology must take as the fundamental working hypothesis", he says, "the assumption that the organic identity of a living organism actively maintains itself in the midst of changing external circumstances." The external stimuli are the changes in external circumstances. The responses are the resulting systemic displacements, which, says Haldane (in general), work actively in the direction of the maintenance of the organism. In general response tends to the maintenance of life, is "to the organism's advantage". If, then, learning is adjustment to a situation of a special order of complexity, it should be to the organism's advantage; and, as we have seen, this is ordinarily so. As was seen earlier in this chapter not every kind of "modification by experience" is ordinarily given the name learning, but in general only such modification as tends to the organism's advantage. Thus one of the most striking properties of learning becomes at once an illustration of the general rule that Haldane has laid down as a biological postulate. The problem of the Direction of change in learned activities, which was seen in the first chapter to be one of the most puzzling with which psychologists have to deal, is thus brought under the canons of ordinary biological thought.

Special Criteria of Learning.—Mention should perhaps here be made of certain other special criteria of learning that have been implied or proposed. In that very able book, *The Animal Mind*, Miss Washburn seems to make a distinction between temporary and "relatively lasting" changes of behaviour, the latter only being classed as learning. "To begin with, we shall distinguish between those modifications which depend on some comparatively lasting alteration in the organism (in its nervous system if it has one) . . . and modifications which are due to a change essentially temporary in its character, in the

physiological state of the organism." [1] In the same way, Day and Bentley state that Modification " is not in itself the equivalent of learning. It suggests learning only when it is preserved by the animal and used as an acquirement upon a later occasion " (1911). In spite of Miss Washburn's important endorsement, I cannot believe that this distinction between changes " comparatively lasting " and those " essentially temporary in character " is scientifically valid. Many changes of behaviour that would undoubtedly be considered as due to learning are of a very temporary nature. Thus, Pavlov stresses the evanescent nature of conditioned reflexes, which are admittedly learned responses. " So infinitely complex, so continuously in flux, are the conditions in the world around, that that complex animal system which is itself in living flux and that system only has a chance to establish dynamic equilibrium with the environment " (1927, p. 15). Here the temporary nature of the organic changes, as corresponding to temporary changes in the environment, is put down as a distinguishing characteristic of learning. If, further, the difference in tempo of various forms of life be taken into account, one organism living and dying in a day, while another may live for a century, the criterion of temporariness or its opposite does not seem sufficiently clear cut to mark a dividing line. Here is hardly a point of division between learned and non-learned activities.

Mention should also be made of another criterion of learning proposed by O. Koehler (1924, p. 590), who claims that the term should only be applied in cases where " a controlling nervous centre is trained by practice ".[2] This is ultimately an anatomical criterion ; it claims that not the training of the organism but the training of a nervous system constitutes learning. One feels that such a distinction, which is one not of behaviour but of the mechanism of behaviour, is in the highest degree arbitrary, and that it ultimately admits that in the behaviour itself no dividing line can be found between what constitutes and what does not constitute learning.

Insight.—The discussion naturally leads to the consideration of yet another distinguishing characteristic that has been

[1] 1926, p. 247.

[2] " Ein übergeordnetes nervöses Zentrum durch übung eingefahren wird." He ascribes to certain unicellulars a " kind of learning " like that of a key that is originally too big and comes to fit the lock. (Experiments of Metalnikof, 1914, and Bözler.)

ascribed to learning, namely what has been called by the Gestalt school Insight. This term is used to denote the organization-into-a-whole of the total sensory and motor field. "A total field would be experienced *without* insight if all its several states, wholes, attitudes, etc., were simply given as a pattern, in which none was felt directly to depend upon any other and none to determine any other." [1] As an example one may give Köhler's well-known experiment in which he placed a banana out of reach of a chimpanzee but with a string tied to it which the animal could pull. This was a simple problem for chimpanzee intelligence. The animals pulled the string and thus obtained the banana. Here there would be said to be insight into the situation, because the animals were able so to organize the total visually given field that the string was seen as a tool wherewith to obtain the banana, the requisite movements thereupon following because of the synthesis of a new moto-perceptual pattern. Such a performance is beyond the capacity of a dog.

In accordance with the postulate laid down in the first chapter, learning of this kind will be considered from one aspect only, namely that of the physiological processes involved. For even though it be granted that there is an organization of conscious elements, yet there must correspondingly be a synthesis of nervous impulses and other definitely physical processes, to which attention and description will be confined. It may be noted that, even granting the possibility of interaction between the physiological and the psychic, whereby one side of the relation could influence the other—and this is a hypothesis to which the present writer cannot subscribe—yet our discussion is not vitiated as long as it is kept on a descriptive level. If no attempt is made to give an explanation of how the psychically effected physiological synthesis comes about, discussion being confined to statement and description of the fact, then at the worst a description in physiological terms will be incomplete as showing the fact and not the cause; unless it be maintained that physiological organization plays no part at all, a proposition which even the most hardy anti-behaviourist will scarcely maintain. The phenomena of "Insight" will then be considered objectively, leaving for others to discuss the

[1] W. Köhler, 1929, p. 371.

relation between the strictly physical phenomena and those known as " conscious ". In this connection attention should be called to the fact that a sudden spurt of learning, indicating perhaps on the neurological side a synthesis of neural processes, cannot be taken as necessarily implying " ideational " behaviour. Still less does the implication follow that such " ideas " [1] as may conceivably be involved affect the physiological processes ; this even though a parallel is thus offered to human behaviour in which ideational synthesis is apparently involved. Since again such an ideational process, even if it occurs, is of necessity at least accompanied by a corresponding physiological process, we shall keep to the surer physiological ground.

Now when such learning takes place as in Köhler's experiment with the chimpanzee, does it not seem reasonable to give the name learning exclusively to the " intelligent " act of synthesis by which the problem is solved ? The animal is faced with the " problem ". For a time he is baffled, then he solves it by suddenly making the right movements. " After a while—the animal being very restless as he was in all similar situations, particularly on finding himself in unwonted isolation—Sultan (Köhler's most intelligent monkey) suddenly went to the tree, climbed up to where the cord hung, and remained quiet a moment." There he pulled a rope, to which was tied a basket containing bananas, several times so violently that a banana fell out, and on repeating this for a second banana broke the rope and carried off the fruit." [2] On repetition of the experiment the rope was at once broken again. Should it not be said that in this experiment, which is not one of the most favourable, the sudden synthesis, whatever it was, was really " learning ", which will then consist of the solving and understanding of a problem rather than the painful fumbling of the animal in a maze or puzzle-box ? The hypothesis is attractive. It seems to bring back the intelligent animal that all animal lovers know, and which has been driven out of the house to have its place taken by a creature of an almost uncanny stupidity who can never do anything right except by the process of doing it wrong first. There is no doubt that the " Insight " animal is more attractive than the " trial and error " animal. There is still less doubt

[1] Cf. Yerkes, 1916. On this point see the discussion of Hunter, 1929.

[2] Koffka, 1924, p. 183.

that the "insight" baby is preferable to that somewhat chilling changeling, the "trial and error" baby.

Fortunately, however, it does not seem necessary to choose between the baby or the animal we love and whose mental processes function by Insight, and the scientific bantling which a generation of faithful scientific observation has given us. They are really the same child. For the maze experiments, supporting what may be called the "repetitive theory" and the Insight theory, are really stressing two sides of the same phenomenon. Learning has been defined as the act of adjustment to a complex repetitive situation. This adjustment may be effected quickly, by a relatively sudden readjustment or synthesis of the neural elements involved, or it may take place gradually. It may be that there are involved in the first case conscious processes which are absent in the second, but there is no evidence that the objective difference is any more than one of speed in systematization. That is to say, there is no evidence of two qualitatively different kinds of neural systematization, one effected by a sudden synthesis, the other by slow accretion. There may conceivably be conscious processes peculiar to a sudden synthesis and comparable perhaps to those concerned in the perception of motion, which, according to Wertheimer, may have as their basis the "physiological short circuiting" of nervous processes induced by the appropriate stimulation of separate retinal areas. The point is well put by Hunter. "Some learning periods may be shorter than others, or may result in a higher degree of efficiency than others, or may involve verbal guidance, but there is no adequate reason for believing in two kinds of learning." [1]

This is not to say, however, that there is no such thing as the phenomenon defined by Köhler as Insight. On the contrary. It seems certain that every act of learning, as well as every simple response, involves such a systematization; for let it be repeated that every organism, even the simplest, acts as a unity towards the highly diversified external flux, so that there must be synthesis, systematization, of such neural or conductive processes as may exist. It is only

[1] The statement of Koffka's that "the improvement of efficiency goes hand in hand with an increased insight into the nature of the task" (1924) is strictly in agreement with the interpretation here adopted.

approximately true that such integration is completely determined by preformed neural pathways, for natural conditions are never twice precisely the same. Synthesis is perpetually in process of operation in every living organism; and the higher the organism, the more delicate its receptors, and the more efficient its nervous system, the more complicated is this continuously varying synthesis, until we come to the human being and perhaps what Kant has called the "synthetic unity of apperception". From the very fact that it is a living organism so reacting that it preserves its own particular life and limb, every creature is perpetually engaged in its own microcosmic act of creation, which brings about the systematization of its own little corner of chaos. The difference between this act of synthesis and that which is ordinarily involved in repetitive learning is that, while in the one case a fresh synthesis, involving organic action, can be accomplished in periods of the order of reaction time, that is to say, from a few hundredths of a second upwards, in the other the synthesis may take a long period to effect. At bottom, however, simple reactions and learning are in the general respect of synthesis entirely similar. In each case a new factor is introduced necessitating a certain organic re-systematization, with a systemic displacement ordinarily called the response. In the one case the new factor, constituting the change of external conditions, is of short duration and occurs once only; in the other the new factor, the total Situation, includes smaller repeated situations, and extends over a longer period of time, while the requisite systematization requires longer to effect.

Repetitive learning thus implies synthesis, which there is no reason to believe is different from that involved in sudden learning. The work of Köhler has performed a most valuable service by focussing attention on the synthetic aspect of learning, the distinguishing feature of the situations he used being that they were near the animal's limit of immediate accomplishment and that the goal could not be attained until the synthesis had been effected in a relatively final form. Synthesis or systematization is characteristic of all organic reaction. It may be of gradual growth, as in what is called repetitive learning, or it may be more rapid, when, if the situation is "hard" and unusually novel, it is called learning by insight. In the latter case there may be unique psychic correlates, with which we are, however, not here concerned.

And even when there is a critical point in the learning series, where sudden synthesis may be held to have taken place, yet repetition is generally necessary for fully efficient performance. Thus in the experiment of Köhler's to which allusion has been made we find that when the test was repeated three days later, Sultan, the monkey in question, at once performed the trick *without the preliminary period of delay*. It seems then fair to say that in all cases that would ordinarily be described as learning we find the organism developing a pattern of action which is, in most instances, improvable by repetition, but which in other cases—those where insight is exhibited—is relatively fully formed at the outset. Admitting the striking nature of Köhler's examples, we shall insist that they at least imply improved performance on repetition. The term learning is ultimately one of convenience. We shall then include the notion of repetition in its definition, and regard any instance which may apparently present a complete and unimprovable act of learning at one presentation—if and when such occurs—as a limiting case.

Learning and Purpose.—As the term is ordinarily used, learning thus implies a series of reponses to successive situations, with successive approximation to a more advantageous response, the total series of such responses forming the Response [1] to the total more inclusive Situation. It has been seen that later responses in general presuppose earlier ones, the total series thus forming a unity in itself. When, further, we speak of learning we generally, perhaps always, imply a certain degree of similarity between the successive presentations of the stimuli; that is to say, there is a certain amount of repetition.

Thus there appears a certain similarity to the formula of the last chapter, where was discussed the nature of the organic systemic displacements characteristic of what is known as "purpose". In each case the series considered is such that later terms imply earlier ones, and in each case the successive terms exhibit a gradual approximation to the end result. The learning series does indeed bear a fundamental resemblance to the total set of actions of an organism which sets out with the "purpose" of obtaining food, goes through a series of part-responses to the various elements of a total situation,

[1] On the analogy of Calory, it is convenient to use Response to designate the unified series of part-responses or adjustments.

each such part-response bringing the final term nearer, until an end result is reached. The fact that learning ordinarily implies an intermitted series of actions, the animal being put into the maze three times a day, for example, while in the intervals it is in its cage, does not constitute a difference between the purposive and the learned activity. For many "purposes" are interrupted in the same way. It is not necessary that the series of actions leading to a "purpose" shall be uninterrupted. But there are differences in the behaviour to which the terms "purposive" and "learning" are ordinarily applied, although here again there is no absolute line of demarcation. First of all, the word learning usually implies the repetition with improvement of at least two sub-situations, themselves purposive. Learning is here, then, the more inclusive term. The cat which has learned to escape quickly from the puzzle-box has done so by escaping; on each occasion that it did so it carried out a purposive series of actions whose end term was the act of escape. It is the totality of all these more or less complete escape-series, the complexity of which is gradually telescoped into a relative simplicity, that constitutes the learning series. "We learn by doing." In the limiting case, however, when learning takes place at one repetition, the purposive series is identical with the learning series, so that here again is a case where no sharp dividing line can be drawn between learning and another general form of response. If we are to say that the chimpanzee learned at one sitting to fit together the two bamboos in order to obtain the banana, it is equally true that the same set of actions was a purposive one, the end term of which was the taking of food.

In the common use of the terms learning and purpose there is another difference which is perhaps worth noting. When we speak of an action as learned, we are speaking of one term of an action-series with reference to preceding terms, while, when we speak of an action as purposive, we are describing it with reference to succeeding terms. To use either method of description implies a context for an action, without which, indeed, the action is not properly described; but the context stretches out in opposite directions in the two cases. A learned action cannot be fully understood without reference to "previous experience". Thorndike claimed that his trained cats were supposed to be more "intelligent" than they

actually were by the onlooker who had not been present at the training experiments. In the same way the psycho-analysts have insisted that many apparently meaningless symptoms of neurotics must be referred to the past experience of the patient, or, speaking in the terms of the present chapter, to the preceding terms of the total action-series of which they form part. To say that the cat is trying to open the door to get food, a usage which has universal sanction, is to include certain actions of the cat within a unity; though the context is here purposive,[1] not one of learning; it is further to speak of certain terms in the total series with reference to the final term, which is the taking of food.

It will be realized, however, that, fundamentally, and perhaps somewhat academically speaking, the differences between "learning" and "purpose" are convenient descriptive distinctions only, corresponding to no functional gap. Both purposive action and learning are instances of the same fundamental process, that of adjustment to a complex of four-dimensional environmental conditions; for practical purposes it is convenient to give the name learning to certain cases where the environmental complex is naturally considered as involving a number of less inclusive "repeated" sub-complexes, and where we wish to consider the stages whereby adjustment is attained as well as the final state of adjustment itself.

Is Uni-receptoral Learning Possible?—A brief account should be given of a problem which cannot be said to be in any way solved, but which raises questions of great interest. It concerns the possibility of modification which ideally involves one receptor field only. This might be called uni-receptoral, as contrasted with pluri-receptoral modification which effects a synthesis involving impulses from two or more receptor fields. Whether such modification should be called learning is another matter. It is really a question of terminology. If the opinion

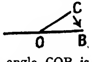
[1] It is repeated that the use of the term "purposive" has here no subjective implications. It is not by an examination of an animal's or a person's consciousness that we are enabled to apply the term purposive to what he does. On the contrary by an examination of his behaviour many psychologists feel that they can make certain inferences about his states of consciousness. The present account prefers to agree on the facts of observation without making the inferences. Cf. Rignano (1931) for an objective account of purpose.

of the present book be adopted, that there is no qualitative break in behaviour between the most primitive response and the most complicated instance of learning, then the problem is one of arbitrarily fixing a point where a term of convenience shall first be applied. The simple action of the amœba to food or to light would be considered by nobody to be learning, while the process by which a child acquires the ability to read would be so styled by everybody. Between these extremes some may prefer to set a dividing line at a specific point; but it is a matter of indifference just where such a line is drawn.

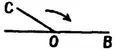
Now there are two broad possibilities for such uni-receptoral modification; the receptor end may be primarily involved, or the central and motor mechanisms. Numerous examples of the latter will be given under the heading "negative adaptation" in the next chapter. The simplest possibility of uni-receptoral modification primarily involving the receptor end is perhaps to be found in the cases of "sensory adaptation" also to be discussed later, and variants of which have been analysed by Hecht in the clam and Adrian in various forms of life. Hecht points out that the sense organs, in their capacity of adaptation, play an important rôle as a buffer between the outside world and the organism (1929). Perhaps at a slightly higher stage of complexity we may have modification by what is known as "sensory organization". It is conceivable that such organization may involve processes in the receptor itself. Evidence for the possibility of such strictly intra-receptoral modification is to be found in Adrian's experiments on the eye of the conger eel (1928) where repetition seemed to improve the connection between different parts of the retina. This result was apparently due to the action of the retinal synaptic layers. However, such organization as takes place within the receptor itself must be of a very rudimentary nature; apparently neural structures of a higher order are generally involved. This would seem to be the case in the development of the "sensory attitudes", as, for example, in Wertheimer's clockface experiment.[1] If two lines are successively exposed with an angle between them like the hands of a clock, with the proper conditions regulating the exposures, not two lines but one line is seen, moving from the first to the second position. This is the well-known Phi-

[1] See Wertheimer, 1925, p. 59.

phenomenon, which is at the basis of what is observed at the moving pictures. Motion ordinarily takes place in the direction of the smaller angle. Suppose, for example, that one line stands on the other as in the illustration.



Motion here takes place in the clockwise direction. If, however, the angle COB is gradually increased, motion will still take place in the same direction, even when the angle COB is



now greater than a right angle. Without the preliminary training the same objective stimulus gives rise to a counter-clockwise perception. "The strength of this working of predisposition showed itself as somewhat different from person to person, and as dependent on the number of predisposing experiments." With one observer the phenomenon could be demonstrated up to an angle COB of about 160°, with another up to one of 130°, and it could even be induced up to an angle of 175°. Now these results are of course subjective, involving as they did the conscious states of the observer, but they must have been correlated with corresponding physiological processes. The seat of this process of physiological training it is in the present stage of our knowledge, impossible to define with exactitude. If Wertheimer's account is accepted, it appears probable that it lies in a part of the central nervous system that is mainly "perceptual" in its function. The training would then be really a "perceptual" one, involving a portion of the central nervous system whose main function is the organization of the impulses originating in the receptor, the "optic sector", to use Köhler's term.[1] Such sensory or intra-receptoral organization plays a large part in Gestalt theory.

Other forms of uni-receptoral modification may readily be imagined. It might, for example, be possible to "associate" a stronger with a weaker stimulus in the same receptor field, so that the latter would finally come to cause a response that originally followed only from the former, something as when a gun-shy dog comes to run away from the distant sound of a shot. The psychology of such cases is at present very obscure, but there seems little doubt of their existence. The whole discussion should be regarded as somewhat tentative.

[1] Köhler's optic sector seems to correspond roughly with Pavlov's "analyser", except that the latter includes also the receptor.

It is possibly the case that some such process is accountable for many phenomena which have hitherto been explained by such *ad hoc* terms as the training of attention. In conclusion it may be said that there is nothing to preclude the possibility that in many cases of "associational" learning such organization also takes place. Strictly speaking such "perceptual organization" should perhaps then be considered as normally constituting only part of a total conservative organic response; in this case it would be parallel to the modification of such a structure as a synapsis. It would be one of the mechanisms of learning, rather than a complete act of learning. Thus when a child learns to read there may be specific organization within the optic-sector, as well as a definite motor-training enabling the right eye movements to be made; just as Beritoff showed that the conditioned reflex involved not only the "analysis" of the motor conditioned stimulus, and its association with the response, but a high degree of kinæsthetic organization as well. Indeed, all evidence seems to be pointing to the probability that learning in the higher organisms will be found to be of a very much more complex nature than has hitherto been suspected.

In the next chapters an attempt will be made to relate the general conception of learning as a complex act of adjustment or response to that of the organism considered as a living system.

CHAPTER VI

HABITUATION

"A fox who had never yet seen a lion, when he fell in with him for the first time in the forest was so frightened that he was near dying with fear. On his meeting with him for the second time, he was still much alarmed, but not to the same extent as at first. On seeing him the third time, he so increased in boldness that he went up to him and commenced a familiar conversation with him." (Æsop's *Fables*.)

When a shadow is thrown on a sea urchin the animal raises its spines. If the stimulus is repeated three or four times visible reaction ceases. Such response has been classified as forming an action-series of the first type, the stimulus remaining identical and response diminishing until it finally disappears. A previously cited example of the same kind of thing is illustrated by the tracing on page 141, which shows the effect of continued tapping of a tortoise's shell over the right leg. The response may be observed to fall off rapidly until the original stimulus produces no visible reaction. The structures concerned are apparently still capable of reaction to the same stimulus, as may be seen by examination of the later part of the tracing, which shows the original tap again producing response after a violent stimulus has been given. The process of habituation has intervened and has been neutralized by the extraneous stimulus.

Several names have been given to the phenomenon; in addition to habituation it has been called, for instance, "acclimatization", "accommodation", and "negative adaptation". These are, of course, *ad hoc* terms; one has still to ask what has happened when a turtle becomes "acclimatized" to a tap on the shell. An answer seems to be suggested from the previous considerations.

In the third chapter organisms were shown to behave in general as would be expected of systems so organized that external changes disturbing equilibrium are followed by intra-systemic displacements leading to a fresh equilibrium which

tends towards conservation of the disturbed pattern. In the example given of habituation the organism was at rest with reference to the environment; this state of rest was disturbed by a change of illumination, but upon persistence of the external change a state of rest was re-established. These cases of habituation then illustrate the fact that when the living system has been disturbed under normal [1] circumstances there are initiated by virtue of the organization of the system internal systemic processes such that a fresh equilibrium is reached. Further, they illustrate very clearly a characteristic property of biological equilibrium. The process tends in general to conserve the biological pattern as it was before disturbance took place. This it does in two ways: the elimination of "useless" movement is economical of the energy reserves, and further, as far as visible movement goes, the original action pattern has been restored. We may say that mechanical equilibrium with the environment has been re-established.

The statement may be generalized. *Habituation is an instance of the living system's power of establishing a conservative equilibrium to change of external conditions.*

Ubiquity of the Phenomenon.—The phenomenon is of very widespread occurrence. According to von Buddenbrock [2] all "shadow reflexes" rapidly degenerate. The assertion has indeed been made that the phenomenon of habituation is generally to be observed on repeated moderate stimulation. (Goldsmith, 1927, pp. 243–4). It has been observed in organisms of all evolutionary grades; in the amœba, the stentor, the vorticella, animals which are all of them single-celled; in the slightly more complex hydra, in worms of various kinds, parasitic grubs, barnacles, sea urchins, mosquito larvæ, bivalve molluscs, various primitive marine animals such as sea anemones, in land and water snails, leeches, frogs, dogs, turtles and fishes, and further in various insects such as ants and spiders.[3] It is a commonplace of animal training

[1] See Child, 1924, p. 236 and elsewhere for a discussion of the meaning of "normal".

[2] Quoted by Hempelmann, p. 456.

[3] For the papers describing the experiments see: Buytendijk, 1928, p. 40; Folger, 1927; Jennings, 1904, p. 229, (amœba); Jennings, 1902 (stentor and vorticella); (the writer has personally observed 'de-habituation' in the latter); Wagner, 1904 (hydra); Hesse, 1899; Hargitt, 1906 (annelids); Yerkes, 1906 (*Hydroides dianthus*); Walter,

technique, whether a "gun-shy" dog or a "motor-shy" horse is being trained out of an undesirable reaction, or a flock of sheep or a herd of cows "learns" to graze on the embankment of a railway undisturbed by the passing of the trains; and in human beings we have the same disappearance of certain responses on appropriate repetition of the stimulus, the lid response dropping away when the auditory stimulus is repeated, the nystagmus or trembling of the eyes, that results from rotation of the body, disappearing when the experiment is performed a sufficient number of times, the so-called "psycho-galvanic reflex" to a bell or buzzer diminishing to zero; even the knee-jerk, if it is elicited often enough and at suitable intervals, shows a tendency to lengthen its reaction time—a step, as the Irishman might say, in the same direction. When we come to activities primarily involving higher centres in the human being, examples are exceedingly numerous from everyone's personal experience. We cease to "notice" the originally insistent bell, the ticking clock, the pressure of our clothes, the noise of the waterfall. Those who live in the city hardly perceive the clash of the traffic, and there is a pretty parallel to this residential habituation in a statement of Piéron's concerning water snails. "Snails living in water shaded by

1907 (planarians); von Uexkull, 1900 (sea urchins); Nagel, 1894 (molluscs); Minnich, 1925 (caterpillars) (?); Goldsmith, 1927, p. 244 (mosquito larvæ); Jennings, 1905 and Kinoshita, 1910 (sea anemones); Wolff, (colony-dwelling hydroids), 1903; Piéron, 1913; Humphrey, 1930 (snails); Gee, 1914 (leeches); Miller, H. S., and Mahaffy, 1930 (fish parasites); the writer and his colleague, Dr. G. S. Melvin, observed habituation to repeated stimulation in frogs, and the writer in the musk turtle, *Aromochlemys odoratus* (experiments unpublished). To these should be added Jones, 1931, psycho-galvanic reflex in infants; Hecht, 1929, an analysis of visual adaptation; Adrian, 1926*a*, *b*, *c*, and *d*, and Bronk, 1929, for various kinds of sensory adaptation as studied through the action current of the nerve. Wells and Hoisington, 1931, have investigated adaptation to pain. To many of these experiments reference will later be specifically made. Many of the above references are given in Miss Washburn's admirable manual. The reader is also referred to the erudite chapter of Piéron (1920, pp. 108–141), where reference is made to studies of adaptation occurring in many other forms, notably the sensitive plant! (p. 112), a statement apparently confirmed by certain experiments of the present writers, various annelids (p. 116), goose barnacles (p. 122), and ants (p. 123). No attempt has been made here to give a complete bibliography.

trees whose branches and leaves move to and fro in the wind hardly react at all to shadows, while very lively reactions would be met in individuals of the same species found in a piece of water absolutely open." It would, of course, be a totally unjustified procedure to assume that the same mechanism was involved in all of these cases. In fact, the contrary is certain. Habituation must be a very different thing in the almost undifferentiated amœba, in the snail with its elaborate system of receptors, nervous system and effectors, and in the almost inconceivably complex human being. On the other hand it would be equally unjustifiable and destructive of all scientific progress to assume a different mechanism for each case. As a matter of fact, the exact nature of every particular mechanism involved need not and indeed cannot here concern us. We are dealing with an elementary conservative phenomenon common to living systems of many different grades of complexity, belonging to them *quâ* self-conserving systems however they may be organized, and it is neither necessary, nor, in the present stage of knowledge, possible to give a complete description of all the processes concerned. It is certain that the habituation of a one-celled stentor to the disturbance of dripping water is not effected by exactly the same mechanism as that of the anemone to the prod of a pencil or that of the turtle to the tap of an electric bell-hammer; equally certain it is that each would be impossible without the existence of protoplasm with its fundamental property of irritability. It is in something the same way that we may recognize that locomotion, digestion, and the ingestion of food are characteristic conservative activities, without understanding their intimate mechanism in every case. Like the phenomenon of habituation, these latter processes are found in organisms with pre-synaptic and synaptic nervous systems, and in those with no nervous system at all; nor, before accepting this fact, do we feel obliged to enquire into the particular method adopted in each case to move from place to place, to assimilate or obtain nourishment. In order to avoid misunderstanding reference should, however, be made at the outset to an important general difference of mechanism, where higher organisms are concerned, in the cases of habituation. In certain cases the phenomenon is apparently due to processes occurring mainly in the receptor system; in others it seems to have a central origin. The former cases are generally said

to be due to "sensory adaptation" while for the latter the term "negative adaptation" may well be reserved. We shall revert to this point later.

Experimental Instances.—In order better to exhibit the nature of the phenomenon under discussion certain experiments will be described in some detail. Land snails of the variety *Helix albolabris*, which is common in the St. Lawrence valley, were placed by the writer (1930c) on an oaken platform about eighteen [1] by five by an eighth of an inch. By means of an electrical attachment this platform could at regular intervals be jerked longitudinally on its ball bearings. The snails were thus allowed perfect liberty of locomotion while they were subjected to an absolutely constant stimulus at regular intervals of time, generally two seconds. On being first placed on the apparatus the animals were allowed a rest during which they emerged from the shell and began to crawl over the board. The electric clock was then started and the jerking began. The first result was a whole or partial withdrawal of the horns (antennæ), at each jerk of the platform on which they rested. As a general rule, to which there were, however, exceptions, the extent of this response gradually decreased, until the jerks were followed by no apparent reaction. Isolated responses might indeed occur after this but they tended to become fewer and fewer, until a series of fifty or sixty stimulations might go without visible movement. "Habituation" had set in. This complete absence of response is very striking in view of the contrast which it presents between the behaviour of the snail at the beginning and at the end of the experiment. When first placed on the board the animal is exquisitely sensitive to such stimuli as vibration of the table and currents of air. A person walking across the floor will often cause a complete withdrawal of the antennæ, the slam of a door in another part of the building may cause the whole body to retire into the shell. At the end of the experiment, however, it may be difficult to cause complete withdrawal. On one occasion a snail totally withdrew for fourteen seconds at the first jerk. At the end of a series of stimulations the table was given a hard blow with a hammer, when only a three-quarter withdrawal followed. Several cases were observed where a habituated snail crawled half over the

[1] The reading 8′ in the paper describing these experiments was due to an error in typing.

edge of the board, so that a portion of its body rested on the stationary table, the rest on the regularly jerking platform. Even in this extraordinarily uncomfortable looking position, with its body being visibly and suddenly bent every two seconds, not a trace of reaction could be seen. This indifference to an almost brutal stimulus was a striking change from the original delicacy of response.

Now it does not seem appropriate to describe what has happened as fatigue, at least as the term is ordinarily understood. To begin with, several cases were observed where the application of an extensive series of stimuli caused only one reaction. One snail, for example, responded to the first stimulus with a semi-withdrawal of the antennæ; sixty-three succeeding stimuli were without results. A reaction such as the half withdrawal of a snail's antennæ can hardly be said to have been fatigued after a single repetition. Such a use of the term fatigue would at least have the wrong implications. Further, as will be seen later, and as is the case with the protective lid reaction in the human being, a more intense stimulus tends to restore the response to the original stimulus; a fatigue that is diminished by more intense stimulation of the same kind seems self-contradictory.[1]

In certain experiments a half-minute interval was allowed during which the animal received no stimulation. At the end of that period the jerk was begun again and response recommenced. But if the process is continued, a half-minute rest will no longer restore the response, so that we feel justified in assuming that there are different degrees of habituation, which might perhaps be graded according to the length of time necessary to restore the response. Let us suppose that the snail, by repetition of the jerk stimulus, has been habituated to such a degree that a one-minute rest now no longer restores the response. If now, during such a period of rest a steel ball is dropped on the platform, movement of the antennæ will

[1] Nevertheless in certain cases there is considerable difficulty in distinguishing between fatigue and habituation. It is possible that some instances of habituation involve in a highly localized form the same mechanism as that responsible for certain phenomena to which physiologists give the general name fatigue. See the heading Fatigue in Bayliss', *Principles of General Physiology*. In view of the fact that habituation and, in general, ordinary fatigue are each of them protective agencies, it would not be altogether unexpected to find them shading off into each other.

again be observed when the jerk begins again. So that the habituation may be removed by rest or extraneous stimulation, and deepened by further stimulation of the kind originally used.

Now these series of facts clearly fit very well the hypothesis that the snail is behaving towards an external stimulus in the way that would be expected of a system so organized as to preserve its mechanical equilibrium with reference to the world without. For consider the animal as it is at the beginning of the experiment. Under the influence of the totality of external conditions it is either at rest or moving as a whole with reference to the environment. In particular the neuromuscular apparatus controlling the movement of the antennæ is in equilibrium with the external conditions in which it finds itself. If these later are subjected to alteration within a certain range, movement takes place until equilibrium is re-established; this may be effected by a sudden jerk, a blast of cold air, a change of illumination. This may be called the primary re-establishment of equilibrium, the body, as already discussed, bringing it about that motion ceases when the disturbing factor no longer operates.

We are here dealing with a single change in the external conditions of equilibrium, resulting in motion. A stimulus, it has been pointed out, is defined by many as a change in the external vital conditions. But what if the stimulus is repeated? If the interval between repetitions is below a certain minimum, the organism will not have time to effect the necessary replenishment of organic material. That is to say the second stimulus will lie in the absolute refractory phase and no movement can result. If, however, the second stimulus lies outside the absolute refractory phase, movement is again possible, and only observation can tell us what will happen if the repetition persists. As we have seen, for certain time intervals, movement actually decreases until it ceases altogether. That is to say, a constant state—equilibrium—with reference to the environmental conditions has again been established. It may be said that the regularly recurring mechanical shock has now become part of the constant environmental conditions. This process may be called the establishment of secondary equilibrium.[1] It may be understood by considering the repeated external disturbance as an

[1] The problem of the relation of habituation to the relative refractory phase is investigated in an interesting paper of L. H. Cohen (1929).

instance of a single temporally extended Situation. This has already been seen to be a fully justifiable procedure. Here, in fact, we have illustrated the necessity of treating the organism as a four-dimensional continuum, a spatio-temporal event. It has been seen that we have just as much right to give the term "situation" to a succession of external events as to a collocation of so-called simultaneous stimuli. It is only because of a faulty abstraction, belonging to an outworn system of physical analysis, that we have in much of our psychological thinking become accustomed to think of a situation in terms of three dimensions, with occasionally the complication of a time element such as "delay". Specifically in this case we may regard the rhythmically occurring series of mechanical shocks as a single Situation, and as a different situation from that formed by a single such shock, just as fifty white spots on a wall present a different situation from one such spot. And just as the reaction to a single spot may be different from that to simultaneous fifty spots, or that to a single person different from that to a crowd, so the reaction to a single mechanical shock and to a series of such shocks may be, and in fact are, different. To say that habituation has taken place is to say that the organism has been disturbed by a change of external conditions, and that by means of intra-systemic displacements a stationary state has been re-established.

Such an account is even more easily seen to be true of continuous than discontinuous stimuli; thus Northrup (1920) found habituation to heat in fishes.[1] A fish transferred suddenly from a temperature of 10° C. to one of 35° died in the course of one to two hours, whereas a fish transferred first to a temperature of 27° for two or three days and then put in water at 35° was able to live indefinitely at the latter temperature. This means that a continuous stimulus has induced changes in the organism whereby certain responses ultimately no longer occur, and although the responses would in this case ordinarily be described as physiological rather than psychological, yet the principle is the same. As the result of the new thermal conditions, intra-systemic changes have been effected which issue finally in an organic stationary state. With the sudden change from 10° to 35° the organism was unable to cope. On a higher plane, fundamentally the

[1] Observed first by Loeb and confirmed by Northrup.

same phenomenon is seen in the habituation of a human being to a continuous noise, such as the roar of Niagara Falls, which finally becomes part of the constant environment to those that live near by. The mechanism by which this result is achieved is very different in the two cases, involving obscure biochemical relations on the one hand, and a highly specialized neuro-receptor system on the other. But fundamentally the same thing has been accomplished in each case. A living system has been disturbed by a new external factor, and intra-systemic displacements have occurred which finally issue in a biological, not a thermodynamical, constant state. Equilibrium has been established, the original reaction having disappeared because of changes effected in the system. From these examples it is a short step to the habituation of a snail to a series of jerks, or of a sea anemone to a series of taps; it is necessary only to substitute a discontinuous external disturbance for a continuous one.

It is now easy to see the intimate relationship between reaction to a stimulus and habituation to a succession of stimuli. Each involves the disturbance of a constant state by an external change; each involves also the re-establishment of a constant state. When the environment returns to the *status quo ante*, we speak here of reaction, when the environmental alteration persists, either continuously or intermittently, we speak of habituation.[1] Thus again in the light of modern physical treatment of space and time it is hard to see any fundamental difference between "reaction" to a spatially discrete manifold, and "habituation" to a temporally discrete manifold, such as a series of taps. This point will be considered later. The burden of proof would seem to be with those who maintain the existence of such a fundamental difference.

We thus have two contrasting situation-response relationships. There is the total situation to which the organism responds in such a way that habituation sets in. This may be,

[1] D. E. Minnich (1925) stimulated caterpillars by means of sound. He found that reaction ceased after an interval of from a few seconds to a minute and attributes the result to sensory fatigue. He was unable to obtain habituation using 5" intervals. It is not of course maintained that fatigue, in the sense of exhaustion of some substance necessary for reaction, or of clogging the machinery of reaction with waste products, is never responsible for the fading away of response.

for example, the total situation comprising twenty-taps-on-the-shell, and the response to it a series of leg-withdrawals gradually becoming less and less extensive until no visible movement is produced. These movements must be considered strictly in relation with each other, and as forming a single unified response to the twenty-tap Situation. This unified response by which equilibrium is established to the total situation is a synthetic, integrative, activity of the organism in process of progressively maintaining its essential unity in space and time.

Contrasted with the total situation is any single tap, with its ensuing response or lack of response. Such a single tap must be considered as forming an element of the total twenty-tap Situation ; as it forms part of the more extensive stimulus complex, response to it is modified. Thus, let us say, the

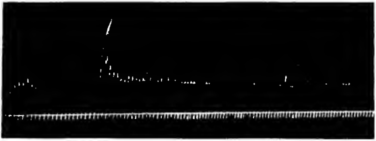


FIG. 3.—Habituation and Dehabituation in the Musk Turtle.

The turtle was clipped to a stand, and to its right front leg was attached a thread which worked a recording lever. The shell immediately over the same leg was tapped every two seconds by an electric hammer, which resulted in withdrawal of the limb at each stroke. There is a noticeable reaction for the first seven stimulations, after which response becomes insignificant. Later the turtle is given two or three sharp taps on the shell near the same place, a small mallet being used. A very pronounced reaction follows, after which response to the electric taps begins again. Nearly two minutes later this has again disappeared, when a second series of sharp taps is administered. This time response to the electric taps is not restored. This falling away of the effect of the mallet blows, similar to one obtained in the writer's experiments on snails, may be due either to the fact that the animal is beginning to become habituated to the mallet blows, or to the fact that habituation to the tap stimulus is now deeper than at first. Possibly both causes operate. (Unpublished tracing.)

twentieth tap, as belonging to a total stimulus-context stretching back into the past, is now no longer followed by visible movement. Further, the organism has eliminated overt response as far as is possible under the existing conditions; which here means that visible response has been completely eliminated. Minimum possible movement is here zero movement. Such elimination "as far as possible under existing conditions" will be seen later to be characteristic of more complex cases also.

Dehabituation.—Equilibrium, such as is established to a series of shocks or shadows and which depends on a nice balance of internal and external conditions, is a delicate thing. It may easily be deranged. We have noted such derangement in the case of the snail, where an intense stimulus, applied after habituation is established, will restore the original response.

The writer has observed a similar restoration of response by extraneous stimulation in the mud turtle (Fig. 3) and the frog. Holmes (1912) reported the same thing to be true of sea urchins. The phenomenon may easily and prettily be demonstrated on a young baby. The hands are clapped behind the child's back every two seconds; blinking occurs several times, but has generally died down by the sixth or seventh stimulation. Habituation has set in. The cradle is then given a sharp blow, and the hands are once more clapped, keeping the proper interval by counting. The child will be observed to blink again. The explanation seems to be that the blow on the cradle requires a new adjustment on the part of the organism which is inconsistent with that involved in effecting the habituation. The peculiar processes involved in the establishment of equilibrium are thus nullified, and habituation has to be re-established. Dehabituation by lapse of time and by another stimulus are thus fundamentally the same, for they involve each of them the derangement of an established state of equilibrium by altered conditions, the alteration being one of increase of environmental energy in the one case, of decrease of energy in the other.

An interesting case occurred in certain of the writer's experiments on snails. Here the steel ball was first dropped not during the period of stimulation or during a rest-pause, but before the snails had been subjected to any other stimulation. The jerk stimulus was then begun, and was followed by responses which increased for a time as the ball

stimulus was repeated. These results at once suggest the objection that what has been termed "dehabituation" is simply an increase of excitability, which, under conditions not known, may summate with repeated stimulation. Such a statement would be in line with the results of certain recent work.[1]

This explanation is, however, not inconsistent with that given in the present chapter, but rather gives a name to the present account, according to which extraneous stimulation does cause the organism to respond to stimuli which have become inoperative. It should be remembered that we are never able to start our experiments on an organism virgin of stimuli and response. We must take the living creature as we find it in a certain environment, into which certain changes are introduced for the purpose of experiment. If these experimental stimuli are of the proper kind, they will disturb any adjustments that have been made to previous conditions, and a condition of greater excitability results. Whether this disturbance consists entirely of the removal of "inhibitory" processes, or whether it involves also the accentuation of excitatory processes, it is impossible to say in the present state of knowledge—nor would it make any difference to our argument.

The Locus of Habituation.—While we have reserved the right to hand over to others the investigation of the specific mechanisms by which a particular vital system tends to maintain any constant, such as the present instance of mechanical equilibrium with the environment, yet, in view of the widespread occurrence of the phenomenon of habituation and the variety of the forms in which it has been observed, a brief discussion of the question of its locus may not be out of place. Jennings (1902) has observed the phenomenon in the one-celled stentor and vorticella, Buytendijk (1928) and Folger (1927) in the amœba. Now the protozoa are not undifferentiated. Many of them are indeed very highly structured along their own lines. Yet these creatures have, of course, neither nervous nor receptor system differentiated. So that in their case we should search for neither a nervous nor a receptoral locus. But here, as in many other instances of unicellular behaviour, we are justified in considering habituation to be the prototype of a function later to be performed

[1] Beritoff (1927) lays considerable stress on increased excitability produced by stimulation, as does V. E. Shelford (1918).

by specialized organs. Again, the phenomenon has been demonstrated, by Jennings and others, in the sea anemone, which possesses a non-synaptic nervous system, and Adrian (1928) has shown that it can take place within the nerve fibre of organisms possessing receptors and synapses. Thus it may exist independently of both receptors and synapses. Further, the remarkable work of Hecht (1929) has exhibited in intimate detail the working of the intra-receptoral process which occurs when an organism becomes adapted to continuous visual stimulation. Primarily by the study of the clam Hecht was able to deduce a formula expressing the relationship between the intensity of the light and the concentration of certain chemicals into which the photo-sensitive substance in the eye is decomposed by light. The overt response of the clam, for example, depends on this chemical relation. When the animal is resting in the dark the chemical system in the eye is in equilibrium. If it is then "suddenly illuminated it responds to this illumination by a vigorous retraction of the siphons. If the illumination is permitted to continue, the animal no longer responds" (p. 229). Hecht claims that when light falls on the animal's eye a reversible chemical process is initiated, that is to say one such that the breaking down of the photo-sensitive substance is at the same time accompanied by the converse building-up process. The incidence of light causes the balance to be inclined in favour of the former, thus releasing certain substances which themselves initiate a second chemical reaction causing a neural impulse. When, however, the light is continued, the reversible process comes to a stationary state. The necessary conditions for the initiation of nervous impulses are lacking and movement ceases. He has been able to put into the form of an equation the situation in the sense organ during adaptation.[1] This equation determines for any given intensity of light the condition in which the sense organ comes to a stationary state. Adaptation is thus automatic, depending on the fact that a certain photo-sensitive chemical system in the eye comes to a state of equilibrium when light of a fixed intensity

[1] $KI = \frac{x^2}{a-x}$. "In this equation the only two variables are the intensity I of the light and the concentration x of the decomposition products P and A produced by it from the sensitive substance S" (p. 268).

continuously falls upon it. Thus this treatment supplements the discussion of Chapter III where it was shown that the eye or visual apparatus may be considered as a partial system of the total living system. The sense organ, Hecht tells us, is not an isolated group of cells: "It is an organ and functions as a whole" . . . "it conforms to what one has always supposed the sensory process to be, namely, a nearly self-contained system, high in energy content, which can be released by a small addition of energy from the outside" (p. 238). Further, as a result of these experiments, Hecht has given us the conception of the sense organ as a buffer between the environment and the organism. By its power of coming to a position of equilibrium specific to persistent environmental conditions, it acts as a protecting layer for the highly responsive nervous system. We have seen that the amœba can come into a state of equilibrium with light of different intensities. In the more highly evolved organisms investigated by Hecht, the visual receptor has, in addition to acquiring the specialized function of visual reception, become also a specialist in the maintenance of equilibrium to external photic conditions.

In addition to Hecht's work, light has lately been thrown upon certain forms of sensory adaptation by the no less remarkable experiments of Adrian and his collaborators.[1] Here an entirely different method was used, namely, that of stimulating the sense organs in the skin or muscle concerned mechanically and by means of several stages of radio amplification recording the action currents produced in the nerve. In this way Adrian claims to have demonstrated in the cat and frog a process of adaptation in the end organs of pressure and "touch" and in those at the base of the hairs. The rate of adaptation in these organs varies, that of pressure being the slowest, that of touch coming next, while that for the organs at the base of the hairs is apparently slightly shorter still. Each of these three receptors, according to Adrian, adapts at a much slower rate than does the nerve,[2] and interesting problems arise involving time relations when one of them is stimulated with consequent neural excitation. Into these problems we cannot, however, enter. Slowest of all in adapting are the receptors in the muscles. So striking is this

[1] See Adrian, E. D., 1926*a*, *b*, *c* and *d*, and Bronk, 1929.

[2] See the diagram, p. 79, Adrian, 1928.

difference in the time taken for adaptation that, following a similar distinction made by Sherrington for reflexes, Adrian differentiates between "postural" and "phasic" receptors, the former being those whose long adaptation time causes them to exert a relatively constant effect on the attached nerve when they are stimulated by a protracted stimulus, the latter, however, being such that protracted stimulation is quickly followed by receptoral adaptation with correspondingly rapid fading away of neural excitation. The receptors for "touch" and those at the base of the hairs are then examples of the phasic class, while the "proprio-ceptors" in the frog's muscle spindle are said to belong to the postural class. Between the two classes is the organ of pressure.

The effect of pain-producing stimuli was studied by the same technique. Such stimuli may presumably be classed as harmful to the organism, and many are of opinion that adaptation occurs only to stimuli that are harmless. Yet Adrian reports, "It is interesting to find that the pain receptors adapt themselves to a constant stimulus at a rate comparable to that of a nerve fibre and much more rapid than that of a complex end organ like a muscle spindle" (1926, p. 48; compare Wells and Hoisington). He later states, however, that certain facts make it difficult to compare the rate of adaptation with that of other receptors (1928, p. 102). However, even though direct comparison of rate is impossible, the fact that there may exist adaptation to a presumably harmful stimulus would make a valuable addition to our knowledge, and will later receive attention in its appropriate context.

The work of Hecht and Adrian has then exhibited in highly convincing detail the working of sensory adaptation. There is, however, evidence which tends very strongly to show that in organisms possessed of a sensory system habituation is not necessarily confined to the receptors. Thus certain experiments in Pavlov's laboratory have apparently located such a process in the dog's cerebral cortex (1927, p. 259). Attention has been called to the fact that adaptation is to be found in the nerve fibre. It will be recalled that it has been demonstrated to exist in the sea anemone. Here its seat is apparently the syncytium or nerve net which is free from synapses, the facts seeming to make it improbable that the responsible organic changes are of a purely receptoral nature.

For the receptor in question is of a highly primitive kind, being apparently not much more than a free nerve ending, while according to report there may be produced in these elementary creatures an adaptational process that will persist for several days. According to the verbal report of Dr. Klugh, sea anemones may become so habituated to constant handling that it becomes difficult to make them respond for demonstrational purposes to a mechanical stimulus.

Further, "In the responses of molluscs to shadows the experiences of one day appear to extend their effects to the following day" (Washburn, 1926, p. 256). According to Piéron, snails became more easily habituated on successive days (1911*a* and *b*). The famous experiments of the Peckhams on the spider (1887) showed habituation to a tuning fork lasting over a period of days. The spider dropped to the ground at the first presentation of this stimulus, but gradually "became used" to it, finally, after some days, ceasing to respond at all Piéron claims that barnacles may become more or less permanently habituated to photic stimuli (1920, p. 122). The exact locus of the adaptational process in these latter cases is, again, uncertain. It would seem, however, intrinsically improbable that adaptation of such long standing should be referred to a receptor, especially in those cases where the receptors concerned are of a very primitive nature. We are thus left with neural processes as an explanation, the synapses probably being involved where they exist. The usual distinction between "sensory adaptation" and "negative adaptation" proper would accordingly appear to be valid.[1] In habituation we seem, in fact, to be dealing with a conservative phenomenon characteristic of living creatures, a phenomenon which may be of very varied complexity, is found in all grades of life, and which may be mediated by many different mechanisms.

Further, according as an organism is more or less highly evolved, it is reasonable to expect that this characteristic organic phenomenon will be found to be effected by means that are of correspondingly greater or less complexity and specialization. Thus where a receptor system and a nervous system have specialized in functions of irritability and transmission formerly effected by the same protoplasm, we

[1] In this book the term habituation is used as inclusive of both classes. Effector "fatigue" possibly gives a third class.

apparently find this highly general organic function, involving cessation of reaction to stimulation, so to speak parcelled out between these two special mechanisms. In higher organisms it is then possible to see both sensory and central adaptation, as evolutionary descendants of the primitive unicellular adaptation. When we come to such highly complex cases of adaptation as the taming and domestication of animals, and human " attentional " adaptation, it is almost impossible at present to talk of a locus at all, except to say that central processes must be involved.

Harmless and Harmful Stimuli.—According to certain writers it is only the harmless stimulus that leads to habituation. Thus Jennings reports that a feeble current of water would at first cause reaction in a stentor, while only a few responded to the second stimulation, and some to the third or fourth. But only when the stimuli are non-injurious does the organism thus cease to respond. Jennings attempted to accustom the animal to the stimulus from a minute quantity of sodium chloride solution. This stimulus was repeated for an hour steadily, and the stentor reacted in each case. There was no indication of its becoming accustomed to the stimulation; a fact, in passing, which seems to show that the first results cannot be thought of in terms of motor fatigue as the term is ordinarily understood. Similar results were obtained by the same investigator when working with the sea anemone; here slight stimuli that are not injurious may cause at first a strong reaction, then on repetition produce no reaction at all or a very small one, but stress is laid on the fact that this is only true of slight stimuli. Similarly Piéron states that various annelids may become habituated to shadows but not to " brutal and dangerous " excitations in the form of blows, " nor even to contact stimuli which constitute more real menaces than do shadows " (1920, p. 117).

Now attention has already been drawn to the fact that the disturbances to which both organic and many inorganic systems can make adjustment must lie within a limited range. If, for example, the pendulum is struck too hard a blow, the system will be disrupted. Again when all the bicarbonate in the buffer solution is neutralized, the buffer effect ceases. And in general the constancy effect is only operative for non-destructive disturbances. Disturbances outside of this " harmless " range destroy the peculiar organization which

is responsible for the requisite compensatory changes. The same restriction must be made for the stimuli to which the living system can adjust. A moderate shock may produce a reaction in an animal, while an intense shock will disrupt the organism and cause death. For all organisms it is then clear that there are certain very intense stimuli to which there can be no question either of response or of habituation. These are indeed harmful stimuli, but it is not this class of disturbance that Washburn and the other writers have in mind. Rather they are thinking of an intermediate group which do not cause immediate disruption of the vital system but which ultimately tend towards its destruction when they are repeated.

Now it seems to be admitted that an intense stimulus is followed more slowly by adaptation than a lighter one, though there appears to have been little quantitative examination of the relation (Piéron, *op. cit.*, p. 116; Washburn, 1926, p. 251; Yerkes, 1906). Danisch, for example, found that a mechanical shock involving the expenditure of 500 ergs required nine strokes to produce habituation in vorticella, one involving 1000 ergs required fifteen strokes, while one involving 1500 ergs was followed by contraction even after forty shocks. Expenditure of 2000 ergs still resulted in contraction after two hours' stimulation with 420 shocks—showing that fatigue in the sense of exhaustion can again hardly be held responsible for the habituation that occurred to less rigorous stimulation. The same author also recorded dehabituation by lapse of time (1921).[1]

In general such a result is to be expected on the hypothesis that adaptation is brought about by intra-systemic compensatory processes tending to preserve the original state of equilibrium. For a more intense external change will require a more pronounced action on the organism's part (see Danisch, p. 142). The more intense stimulus will be one that will produce a greater derangement of equilibrium, which it is reasonable to think of as requiring a more persistent compensatory effort on the part of the organism before the original state is restored. It must nevertheless be insisted that this is a reasonable supposition only, and one which lacks any thoroughgoing and rigorous quantitative proof.

Taking a series of stimuli that are gradually increasing in intensity we should thus expect to find a corresponding

[1] The control of these experiments is perhaps under suspicion.

increase in the time necessary for adaptation, with possibly a limit of intensity, as was found by Danisch, beyond which adaptation is impossible. This last class of stimuli would presumably be of an "injurious" nature. It would include Piéron's "brutal and dangerous" excitations. There are, however, further complications. Adaptation may take place to painful stimuli, which are presumably for the most part injurious. It may also take place to undeniably fatal stimuli, as is shown by the well-known experiment where a frog is gradually raised to a fatal temperature without exhibiting any unusual reactions. It is further possible, by arranging a sufficiently long time interval, to bring about that adaptation does not take place to undeniably harmless stimuli (*e.g.* Hargitt, 1906).[1] So that the teleological criterion of "harmfulness" is neither necessary nor sufficient. In the absence of further evidence the most conservative statement seems to be that under certain conditions adaptation will take place to certain stimuli but not to others, and that in general, possibly by a process of selection, the former group contains the harmless stimuli, the latter the harmful ones. Further, at least an important group of harmful stimuli are of a very high intensity. To this general statement, however, there are doubtless many exceptions. When these occur, as in the case where the organism adapts to a harmful stimulus, it must be concluded that Nature has made a mistake. For in spite of the assertions of the romanticists, Nature is by no means infallible.

Certain interesting problems are raised by the fact that, other things being equal, a more intense stimulus apparently does require a greater number of repetitions for adaptation than a less intense one. The question of the exact mechanism of non-sensory adaptation has been left unanswered, and is perhaps at the present stage of knowledge unanswerable. It is certain, however, that where such adaptation occurs the one stimulus must exert upon the organism some effect that tends against response of the same organism to the same stimulus repeated at the appropriate interval of time. It is

[1] Miller and Mahaffy (1929) give an interesting example of variation between stimuli in the time needed for adaptation. In the parasitic Cercaria Hamata habituation could be produced to 1 or 2 repetitions of a shadow, while 79 mechanical stimuli were necessary in one case to reduce reaction to a few vibrations. They suggest separate mechanisms.

the nature of this effect that is uncertain. Now the closest parallel is that of the physiological process of inhibition, which is the process whereby a reaction to one stimulus is impeded by the action of a different stimulus, that is to say, one affecting another receptor, though not necessarily another kind of receptor. For example, " Reflex inhibition of the flexors of the knee (spinal dog) is regularly excitable by stimulation of the skin of a digit of the crossed hind foot " (Sherrington, 1911, p. 134). That is to say, reaction of the flexor muscles is checked by a stimulus acting at a different point. But there is good evidence that, like its counterpart the excitatory process, the inhibitory process grows in general more intense as the stimulus itself increases in intensity. Sherrington again states that " increase within limits of the strength of the stimulus of the inhibitory afferent increases the amount of relaxation which the inhibitory reflex can produce in the contraction of a given excitatory reflex " (1925). Heymans, working with visual, olfactory, and auditory sensations established the fact that a more intense sensation exerted an inhibitory action on a less intense one, and was able to express his results quantitatively in the law that this effect was proportional to the inhibitor (1899). Spencer (1923) trained rats to react to a weak light, and then introduced a stronger one, which he found to exert upon the already established reaction an inhibitory effect proportional to its intensity. These results are, in the main, in line with those obtained in Pavlov's laboratory on dogs. A doctor's thesis of S. H. Fursikoff [1] published from Pavlov's laboratory makes it clear that a more intense degree of inhibition was produced by a very strong stimulus than by a weak one, and the same general result is noted by Bechterew (1926, p. 254) for human reactions. The results of these workers seem to point to the general conclusion that any inhibitory effect attaching to a stimulus will increase when the intensity of the stimulus is increased. An extreme instance of an intense inhibitory state following a stimulus of a very high degree of intensity is

[1] Fursikoff, D. S., *Interrelations between Excitation and Inhibition*. See Bibliography of Pavlov's *Conditioned Reflexes* (22), p. 423. The translation was with characteristic generosity loaned to the writer by Professor H. S. Liddell of Cornell University. Pavlov's statement concerning stimuli of medium intensity does not seem explicable in view of Fursikoff's study. See his *Conditioned Reflexes*, p. 388.

perhaps to be found in the Charcot method of inducing hypnosis where catalepsy and allied phenomena were brought about by such methods as the sudden banging of gongs. Following then the parallel of the only other organic checking process of which we have exact knowledge, namely, the process of physiological inhibition, it would seem reasonable to expect that any checking process exerted by a stimulus upon the reaction to a repetition of the same stimulus would increase in intensity with increase of stimulus intensity. But here a paradox seems to emerge. Experiment appears to show the opposite. A very intense stimulus, as we have seen, may not be followed by habituation at all!

This is one of these cases where an apparent exception really throws fresh light on the point at issue. The solution of the paradox lies in the fact that fading away of response depends not on the absolute intensity of the physiological process of check, but on its intensity relative to the process of excitation; as the intensity of this latter rises the checking process appears not to keep pace with it. The point is treated by Sherrington, who worked with the scratch reflex and obtained a phenomenon similar to that here termed habituation.[1] "In my experience", he says, "these spinal reflexes

[1] Sherrington implies a protest against the uncritical use of the term "fatigue" (1911, pp. 222, 223). "The waning of a reflex under long maintained excitation is one of the many phenomena that pass in physiology under the name of 'fatigue'. It may be that in this case the so-called fatigue is really nothing but a negative induction. Its place of incidence may lie at the synapse. It seems a process elaborated and preserved in the selective evolution of the nervous machinery." And later "the loosening of the hold upon the common path by so-called 'fatigue' occurs also in paths other than those leading to muscle and effector organs. If instead of motor effects sensual are examined, analogous phenomena are observed. A visual image is more readily inhibited by a competing image in the same visual field when it has acted for some time than when it is first perceived (W. MacDougall)." In spite of the fact that the phenomenon observed by him was located at a relatively low level of the nervous system, for Sherrington the term "fatigue" is plainly one of classification only, without further implication. If fatigue is involved, it is a special kind of fatigue, a phenomenon *sui generis*. This is the position taken by the present writer, who prefers, however, the term "habituation" as less likely to cause misunderstanding. Thus Sherrington's use of the term "fatigue" does not imply that he was dealing with a different phenomenon from that here termed habituation. See Kafka (1922, p. 86 *seq.*)

fade out sooner under a weak stimulus than under a strong one. This seeming paradox indicates that under even feeble intensities of stimulation the threshold of the reaction gradually rises, and that it rises above the threshold value of the weaker stimulus before it reaches that of a stronger stimulus. . . . We have to do then with a process which may often be, relatively to the *production of the natural discharge*, greater than when a stronger stimulus is employed" (1906, p. 220. The italics are those of the original text). The facts thus fit together fairly well. Both in the experiments on intact organisms and in those of Sherrington on isolated structures at a relatively low neural level, it must be assumed that under appropriate conditions a stimulus exerts some kind of checking effect upon the reaction to a succeeding similar stimulus. This effect may vary in its efficacy from stimulus to stimulus,[1] for reasons not yet established, and of course from species to species. It may also vary within the same receptor with variation in the intensity of the stimulus. As the latter becomes more intense, the checking effect, whatever it is and wherever it is located, gradually increases in intensity, that is to say, gradually becomes capable of preventing reaction to more intense stimuli; but it does not increase as rapidly as does the exciting effect. Consequently, as the latter grows more intense, the checking effect must in general be deepened by more and more repetitions in order to induce habituation, until a limit is reached where habituation does not take place at all. Such a stimulus is likely to be harmful to the organism. The picture is, of course, to a certain extent schematic, but it seems to combine in a reasonable way the results of several lines of experiment.

Higher and Lower Levels of Organization.—From the experiments of Sir Charles Sherrington described in the last section there is a natural transition to a point raised by Professor Dodge in his monograph entitled *Elementary Conditions of Human Variability* (1927). The subjects of Sherrington's investigations were spinal dogs, that is to say, animals in which

for an interesting discussion. Where it is not merely a clogging process, fatigue is, of course, a protective (conservative) mechanism. When Sherrington contrasts the cessation of a reflex under "fatigue" and under inhibition, he means, of course, inhibition from another source of stimulation.

[1] See *e.g.* Miller and Mahaffy, 1930, and Yerkes, Ada, 1906.

the higher parts of the central nervous system were rendered inoperative. The seat of the physiological process involved is thought by the illustrious investigator to be "intra-spinal and central . . . it affects the afferent part of the arc inside the spinal cord, probably at the first synapse." [1] Now a casual inspection of Sherrington's diagrams shows that the decline of the reflex, though progressive, is much slower than that of the responses with which we have hitherto been dealing. The snail, for example, often ceases to respond after one stimulation, and in the same way Ada Yerkes reports of *Hydroides dianthus*: "In the greater number of these tests with different specimens the animal responded only the first time or possibly one to three times and then gave no further response throughout the ten trials." Similar results are reported by many investigators. Sherrington reports indeed that different kinds of reflex have each their own characteristic speed of decline, and that a similar statement is true of the same kind of reflex under different physiological conditions.

But even so, the difference to which he alludes is not great enough to allow of the possibility of the disappearance of response in from one to three trials, it being remembered that in Sherrington's experiments several hundreds of repetitions were necessary. Some other explanatory factor must be sought, and it is apparently to be found in the fact that Sherrington, as it has already been stated, was working on a relatively low level of the dog's nervous system, a level, in fact, whose chief function in the body seems to be the engineering of automatisms. The spinal level is the home of the stereotype, and it is remarkable that Sherrington has been able to show even so much variation in a purely spinal action. For variation of any considerable magnitude we must go higher in the nervous hierarchy, which is what Dodge has done, and his work is the more valuable in that he was able to compare results at various levels of the same organism. The subtitle of the work is "A Study of the Variation of Successive Responses to Similar Stimuli at Different Levels of the Cerebro-Spinal System." In fact (p. 105), "One may venture the hypothesis that the effects of repetition increase progressively from lower to higher systematizations." As this remarkable series of experiments is of fundamental importance in the theory of learning, it will be described in

[1] *Loc. cit.*, p. 218.

some detail. The subject was an intact human being. "The selected processes were recorded mostly by photographic methods, in a series of experimental sessions lasting about twenty-one months." During this period there were studied the knee-jerk, and the lid reflex which takes place to an auditory stimulus together with other processes that do not immediately concern us here. Later a study was made of the compensatory movement of the eyes which occurs when a human being is rotated. These three responses give cross sections of the functioning of the human nervous system at three different levels, the knee-jerk at a low level of the spinal cord, the lid reflex higher up in the "Fourth Ventricle" of the brain, the eye movement at a still higher structure of the brain, the pons. As is to be expected from this fine experimentalist, the technique was irreproachable, all the processes studied being registered with extreme accuracy by photographic methods. For the details as to methods and results the reader is referred to the original monograph and the papers to which it makes reference.

Records were taken of the knee-jerk, each showing the reaction time (latency), and extent of two such responses separated by half-second intervals. Twenty-four such records were taken each day, so that a complete photographic description of forty-eight knee-jerks was secured for each working day of the total experimental period of twenty-one months. Thus 1032 knee-jerks were studied in all. By averaging the first and second reactions throughout the whole period the following figures were obtained.

TABLE I

Comparison of the Latency and Extent of Paired Knee-jerks

	Latency	Extent
Average for first reaction .	40·5 sigma [1]	12·5 mm.
Average for second reaction, one half-second later .	40·4 sigma	8·0 mm.

Thus, taking the average, the first stimulus of each pair produced no effect on the latency of the second reaction, that is to say, on the time between the stimulus and the response, but the average extent of the second reaction was diminished

[1] By Professor Cattell's convention, sigma is used to express an interval of 1/1000 sec.

by about one-third. This shows the average effect of repetition at half-second intervals. Taking, however, the unaveraged figures for the entire experimental period, which give the progressive effect of repetition at twenty-four hour intervals, the opposite is the case. An appreciable slowing down was observed as the experimental period progressed, without, however, any change of extent.[1] That this change in speed of a very elementary human response which was " practised " over a period of twenty months indicates progressive physiological changes in the organism, there can be no doubt ; how far these changes are identical with those responsible for phenomena observed in less highly differentiated organisms, it is impossible to say, though the burden of proof undoubtedly lies with those who maintain that there is any radical difference. The data show the intimate and complicated relation that exists between the problem of the refractory phase and that of habituation, other experiments seeming to indicate that one of the effects of practice may have been a lengthening of the refractory period.

With the eyelid response we approach nearer that part of the human nervous system which is usually considered to be the seat of " learning ", namely the cerebral cortex. " As in the case of the knee-jerk, approximately 1032 records are available for the experimental period." The averages of all these records are expressed in Table 2.

TABLE 2

Comparison of the Latency and Extent of the Paired Lid-Reflexes

	Latency	Extent
First reaction	39·4 sigma	7·9 mm.
Second reaction one half-second later . . .	40·5 sigma	2·1 mm.

The table shows that the amplitude of the response is diminished by about 73 per cent. Now it is known that the refractory phase of the lid reflex may last about three seconds, so that this diminution of extent seems to be due to the fact that at the time of the second stimulation the structures

[1] This difference suggests the possibility that receptoral changes may have played a greater part in the effects of repetition at half-second intervals.

concerned were still in a refractory state consequent upon the first. There is a striking contrast with the corresponding figures for the lower level, where a half-second interval has the effect of reducing amplitude by only one-third; *repetition under similar conditions has more than twice the effect in reducing amplitude of response at the upper than at the lower level of the human nervous system.*

It is possible here to see a trend in physiological organization which, when continued, will ultimately bring us to the physiological correlate of some such function as memory. For while it would clearly be absurd to speak of " memory " at the level of the knee-jerk or even of the lid reflex, and while we are here dealing with purely physiological mechanisms, yet at each level we have one physiological event influencing another subsequent to it, with this effect much more strongly marked at the higher stage of organization. With certain reservations it is characteristic of higher forms of life that their behaviour is integrated over longer periods of time; and this possibility of relating events in the life history of an organism over longer time intervals corresponds objectively to one factor in what is meant by the possession of a " better memory ". The devil in disguise, it will be remembered, asked the nigger as a boy how he liked his eggs, and disappeared. Fifty years later the devil reappeared and the immediate response was " poached ". The fact that we can group these two events together as question and answer over fifty years entitles the organism in question to be assigned in this respect a better memory than, let us say, Professor Hunter's dog, who after a delay of five minutes " forgot " which of the three chambers carried a light.

The same trend is shown by the following table which exhibits a comparison of the first five and the last three sessions of the entire experimental period.

TABLE 3

Comparison of the first five Sessions of 1915 *with the last three Sessions of* 1917

November 17–December 17, 1915:	Latency	Extent
First reaction, average . .	37·0 sigma	11·2 mm.
Second reaction, average . .	39·6 sigma	3·6 mm.
July 5–July 7, 1917:		
First reaction, average . .	40·3 sigma	5·2 mm.
Second reaction, average . .	43·4 sigma	1·3 mm.

Thus there was a strongly marked decrease in extent of reaction over the whole period, contrasting with the knee-jerk, which remained unchanged under the same conditions. The same effect showed itself during each series of records and during each four-hour session. That is to say, the total series included a number of smaller series, in each of which diminution of response was apparent.

This tendency towards increasing effect of repetition is still more marked when we come to a still higher level, that of what is called the compensatory deviation of the eyes. When a human being or other higher animal is rotated, the motion of the environment relative to the organism causes a rapid to-and-fro movement of the eyes, which has as its counterpart in ordinary life the tendency to keep the line of vision directed towards the same point. This result is effected by a dual control. There is the more primitive, rougher response originating in the stimulation of the semi-circular canals. This can give only a first approximation to the required accuracy of fixation ; the final adjustment is made by means of vision. The practice period here lasted only six days, the subject being rotated each day in the same direction at a speed as nearly uniform as possible. The nystagmus or rapid oscillation of the eyes was photographically recorded during and after rotation. Post-rotational nystagmus decreased from day to day and from record to record each day. Nystagmus during rotation decreased from record to record, and from day to day. " During the last three days all records after the first show that the amplitude of nystagmus reached zero." Here we have habituation reducing movement to a zero point within a week.

Twenty months' experiments on structures at the two lower levels show approximate reductions of movement by one-third and three-quarters respectively ; four days' experiment on structures at the higher level show its complete disappearance.

Although involving structures that are relatively high in the nervous system, the last reaction studied is still, of course, a comparatively primitive response. Higher activities were studied during the series of experiments described in the Dodge monograph, and notably the process of " memorizing " a series of words which were exposed backwards letter by letter. The subject reacted to each word by reading it, and memorization was measured by the fact

that fewer and fewer letters were necessary for identification as the time progressed, until finally the entire series was learned. Such an activity, of course, involves the highest nervous centres of all. As might be expected from the general trend of the results previously described, modification of response was observed to take place to an extent even greater than before. In fact, whenever the same stimulus series was employed twice, whatever the time interval, there was observed a modification in the second response. Even though the time between the two was measured in months, response to the "first" situation had a noticeable effect on that of the "second"—a cerebral *tour de force* which, when one compares it with the performances of less gifted organizations hitherto considered in this chapter, must be regarded with feelings not far removed from awe. But there is a striking difference between modification of response at the level of the cerebral cortex and all other instances previously considered.

While in the experiments hitherto described the general result of repetition, with the conditions of stimulation actually employed, was to increase reaction time, at the highest stage of all repetition was always accompanied, in Dodge's experiments, by decreased reaction time. At the one set of levels we have, with the conditions of experiment employed, a gradually increasing tendency towards elimination of response when stimulation is repeated, accompanied by a tendency towards lengthening of latency; [1] at the highest stage of all we seem to have a tendency towards conservation of response with decrease of latency. It is, indeed, a more or less accepted principle in psychology that "reaction times" in general decrease with practice.

It would, however, be too hasty a conclusion to assume that Dodge's [2] experiments had established a fundamental difference between the effects of repetition at higher and at inferior levels. For the stimuli were carefully selected. We have no reason for believing that stimulus-series of every range of intensity and temporal interval would have given the same results at the sub-cortical level; indeed, from what

[1] An exception is found in Table 1, where, however, the latency is practically unaltered.

[2] Professor Dodge, of course, would never intend this interpretation of his results.

we know of the refractory phase and the supernormal period following it, it seems possible that experiments could have been devised which would give different results, at least for short intervals. The experiments thus furnish no evidence that repetition of stimulus at the lower levels of the intact human subject must without exception bring decrease of reaction-extent, and increase of latency. Equally unjustified would be the assertion from these experiments of a contrasting rule that repetition of stimulus at cortical levels is invariably followed by conservation of response and decrease of latency. For to begin with the reaction-time experiments on which such a conclusion might conceivably be based have also been carefully selected, out of the experimenter's knowledge of human nature, to produce such conservation of response. But experiments might be expressly devised which would use the cortical level to eliminate response, for example, to train a subject to " control himself " when a distracting stimulus was presented. There is no doubt that such experiments would succeed; the physiological textbooks inform us indeed that a large part of the function of the higher centres is to exercise exactly such inhibitory control over actions which would otherwise be carried out.

As a matter of fact, conservation of response was expressly attained in these experiments by means of what is ordinarily called a " motive ". For consider the conditions of experiment of the two levels. In the one case a simple stimulus, such as the snap of a rat-trap, initiates impulses which travel along paths preformed from birth. One external receptor field is alone involved, though of course the kinæsthetic organs in the eyelid muscles are stimulated. With this simple state of affairs contrast the response of " reading aloud " a " printed word ". There is the motor side, consisting of the highly elaborated speech response which has been integrated by the process of " learning to talk ". There is the equally complex sensory-motor process known as " learning to read ". Even granting that the requisite training has taken place, it by no means follows that the subject will read the word out loud when he sees it printed before him. Because he can, it does not follow that he will do so. There are only a few of us who read aloud the subtitles at the moving pictures. To ensure that the reading-aloud process will take place in the experiment, the subject must receive some additional stimulus,

either special instructions, or the general laboratory " set " or something else of the sort. The conditions of stimulation corresponding to this " set " or these " instructions " are of very great complexity. Further, the reading out aloud would not necessarily persist from experiment to experiment unless the original instructions were to the effect that such was to be the case, representing a further refinement of stimulation, or unless the instructions were given each time. The " instructions ", again, are obviously nothing but auditory or visual symbols entirely inadequate in themselves to produce the very complicated response that actually does follow them, and depending for this efficacy upon prolonged education. Even then there must be a " motive " for " obeying the instructions ".[1] Clearly the man in the street would not submit to the ordeal of reading these series of words under these rather uncomfortable conditions unless it were somehow made " worth his while ". It is plain that there is a very great difference between the unlearned, simple response to the clap of the hands behind a child's back and the amazingly complex performances of which this experiment of Dodge's is a sample. We should have, in fact, no right to compare the two as exhibiting at different sets of levels contrasting " effects of repetition " because we do not know what fresh factors have been introduced into the more complicated response. It is clear, however, at least that this whole performance at the level of the cerebral cortex is intimately related with what in the absence of a more objective term we have called " motive ", which was absent from the experiments at the subcortical levels. It is then possible that the response was conserved because there were, extraneous to the response itself, factors affecting its conservation, in which case if there were any tendency towards disappearance of reaction it may have been masked by the very conditions of the experiment. Apart from differences of complexity and the fact that responses are " innate " in the one case and " acquired " in the other, in order to compare these effects of repetition upon nervous structures at cortical and subcortical levels the " motive " must then be eliminated.[2]

[1] Thus some laboratories find it advisable to use paid subjects.

[2] The term motive is popularly used to suggest that an action is being performed not because of the immediate situation but because of some environmental feature that will, perhaps, occur subsequently. The

Here should be recalled the fact that there are recorded cases of habituation where the onus of proof is certainly with those who might claim that the cerebral cortex was not involved. There should be mentioned first of all the phenomenon of the "extinction" of the conditioned response, which will later be seen very probably to involve the same mechanism as habituation, although perhaps in a more complicated form. Extinction is indeed exactly the repetition of a (conditioned) stimulus with the elimination of the motive which was, in Pavlov's experiments, food. Conformably with what might have been expected from the previous discussion, response falls away. Yet there are here complicating factors which will be discussed later, and which might hardly allow us to adduce extinction as an example of habituation pure and simple. Apart from extinction, however, habituation in intact dogs has been obtained by Beritoff (1927), Popoff [1] and Rosenthal (1929). Pavlov, in fact, claims that repetition of any conditioned stimulus is followed by inhibitory processes weakening further response. When the unconditioned stimulus is presented at the same time, this effect is then masked, but nevertheless still present. Thus according to Pavlov's theory what is ordinarily called the motive would be expected to act in precisely the direction of covering up habituation. So that Dodge's results should not be adduced as showing that the effect of repetition is different according as higher or lower centres are involved. With motive for the contrary eliminated, repetition of stimuli within certain ranges of intensity and temporal interval may apparently be followed by the same general result at the highest as at any other level, namely, degeneration of reaction which may be more or less complete. When conditions are really parallel any

term is not strictly scientific, as Woodworth points out, and can perhaps not be made so by any delimitation of its meaning. It is, however, often useful to call attention to the fact that the total context and the physiological condition of the organism must be taken into account when seeking to explain behaviour. Roughly it may be said that when the immediately present environment seems obviously incapable of explaining reaction and on the other hand the contextual element seems necessary before we can explain it, then we ordinarily speak of motive. Why is the hungry animal thus bounding over the stream? Clearly not solely "because" of any inherent property of the stream, but because of the food on the other side

[1] Reported by Pavlov, 1927, p. 255.

difference in response to repeated stimuli at the highest and the lowest levels appears to be one not of quality but of degree. This is not, of course, to say that all repeated stimuli result in reactions which fade away unless there is a "motive" to conserve them. It has several times been pointed out that such a statement would certainly not be true. Difference of stimuli, differences of intensity, differences of temporal interval, and probably other factors must be assumed to effect a difference in the habituation rate, which may ultimately amount to a total absence of habituation. It does, however, imply that the phenomenon is not necessarily incompatible with participation of the highest neural processes in the more advanced organisms. This result was perhaps to be expected. As a characteristic reaction of living things, a special case of the general property of conservation, it would indeed be surprising if we were to find that habituation could not be effected by the most highly organized of known living structures. All the evidence goes indeed to show that the process of adjustment to the outside world, like all others that are accomplished by the central nervous system of the higher organisms, is here effected with an astonishing degree of finesse, elaboration and intimacy.

In summary it may then be said that habituation affords an instance of the process whereby a living system re-establishes a conservative mechanical equilibrium with the environment when equilibrium has been disturbed. It is found at all grades of life, and in the higher living forms it may be effected by specialized structures such as the receptors and, apparently, the central nervous system, when it is known as sensory adaptation and negative adaptation respectively. There is no rule which will determine whether it will take place in a given organism for stimuli of a given intensity, quality, or temporal interval. Teleological considerations do not seem to apply.

Practical conclusions are at this stage a little premature; but it is not often that a moral may so easily be pointed against a copy-book maxim, which has also the support of a certain body of psychological opinion. Granting all the conditions for habituation, repetition of stimulus will not make for conservation of response. What these conditions are, we do not know as yet with exactitude, but it may well be that they are more often fulfilled than is recognized in

everyday life. When they do obtain unmotivated repetition will necessarily defeat the end of conservation of response. Quite apart from any effect of "punishment" in diminishing reaction, practice does not necessarily make perfect, as many thousands of school children have found to their sorrow. Motivated practice may make perfect, unmotivated practice will very often render imperfection still more imperfect. "Oh most sad, that dry drudgery of the desk's dead wood." If by the work of those whose researches are cited in this chapter the intolerable boredom of endless classroom repetition may ultimately be lightened for only one schoolboy, they will, one may suspect, feel that their labours have been most fully repaid.[1]

[1] Reservations as to the law of exercise have of course been expressed by Thorndike (1931). To Dunlap is perhaps due the reopening of the entire question. See his vigorous paper (1928).

CHAPTER VII

THE TRANSITION TO ASSOCIATION

"How much work has been done entirely in vain because we have not as yet fully appreciated the fact that in the absence of the mechanism for association nothing but the biologically adequate stimulus can bring movement." (Edinger.)

In the last chapter the phenomenon of habituation was treated as an instance of the organism's self-conservative tendency. Being thrown out of equilibrium by various classes of repeated disturbance, the living system initiates compensatory processes whereby conservative equilibrium, a constant state, is again attained. Such disturbances are in the higher organisms initiated by organs specialized for the purpose; and habituation was considered as involving stimuli from one receptor field only, giving an example of what was called in the fourth chapter an action series of type A.

Here it is advisable again to draw attention to certain facts which have already been discussed in a preceding chapter. Association, at least in its simpler forms, was seen typically to be concerned with at least two receptor fields, giving an action series of the third type. The dog secretes saliva because his gustatory and auditory receptors have been simultaneously stimulated. The rat threads his way through the maze in quicker and quicker time because his contact, visual and other receptors have been excited in a certain way immediately before food, although he later comes to dispense almost entirely with all but the internally excited proprio-ceptors. Such combination of receptor function is, of course, a great advantage for the organism in its task of self-conservation, although, as we have seen, the claim of certain writers is probably not justified that the appearance of associative memory marks also the appearance of a qualitatively different stage of evolution. The process of association may now be considered from a slightly different point of view. Consider a multi-receptoral organism of comparatively simple type. A dog is

stimulated uni-receptorally by the sound of a metronome or an electric bell. These stimuli at first provoke what is called in Pavlov's laboratory the "orientation" reflex, well known in all domestic and experimental animals. The animal turns towards the stimulus, he "is interested", "pays attention", we say. On repetition of the bell, however, this "interest" grows less and less marked, until he finally pays no attention to the sound at all. Habituation has set in, as discussed in the last chapter; in the terms of Pavlov's laboratory the orientation reflex has been extinguished (Rosenthal, 1929). If the process is continued, sleep results, in all cases according to the experimental account.

Suppose now the dog instead of being intermittently stimulated by the sound of a metronome or a bell is given meat powder at similar intervals. Then he will react in the manner appropriate to this second kind of stimulation, namely by salivation, gulping down the powder, and so on. If the doses of food are kept small enough, it is to be suspected that he will keep awake longer than when he was stimulated with the bell, although, as far as I know, the experiment has not been performed. If, however, he is given a large enough quantity of food, he will again probably go to sleep.

The stimulation of these two different receptor fields has supposedly been effected on each receptor separately. What if they are conducted simultaneously? It is in this case that what is called a conditioned reflex will be formed, provided that certain conditions are fulfilled which need not concern us here. Under these circumstances, the ringing of the bell will finally cause a reaction originally set off by food. The animal is now "interested" indeed, but in the food, rather than the metronome. He "pays attention", not to the source of the sound, but again to the food. Thus for a conditioned reflex we here have two receptor fields involved, with two separate responses. After the formation of the conditioned response, food-response occurs to either of the two stimuli. The orientation response has disappeared.

A remarkable instance bringing into relief this fact that the conditioned reflex involves two stimuli and two responses, is related by Pavlov. "Cauterization or pricking of the skin deep enough to draw blood was made to acquire the properties of an alimentary conditioned stimulus. These experiments have been apt to upset very sensitive people; but we have

been able to demonstrate, though without any pretension of penetrating into the subjective world of the dog, that they were labouring under a false impression. Subjected to the very closest scrutiny, not even the tiniest and most subtle objective phenomena usually exhibited by animals under the influence of strong injurious stimuli can be observed in these dogs. No appreciable changes in the pulse or in the respiration occur in these animals, whereas such changes are always most prominent when the nocuous stimulus has not been converted into an alimentary conditioned stimulus" (1927, p. 30).

Suppose now that the two stimuli in question call out the same response. Such an experiment has been performed by Ada Yerkes in the paper to which allusion has been made. It provides an intermediate stage between habituation, which involves one receptor, one type of stimulus, and one response, and association of the conditioned reflex type, with an ordinary complement of two receptors and two responses. As occupying a unique place among investigations on learning, this experiment will now be considered in greater detail.

Mrs. Yerkes worked with a tube-dwelling worm, *hydroides dianthus*. The first stimulus used was a shadow. Ten trials were given with this stimulus. "In the greater number of these tests (sixteen out of twenty-seven) with different specimens the animal responded only the first time or possibly one to three times, and then gave no further response throughout the ten trials." "Evidence from a later series of the same experiments tends to show that fatigue is not the cause, since the animals are capable under stronger stimulation of contracting a large number of times in rapid succession without apparent fatigue." Afterwards the shadow was given together with a touch stimulus, the latter always inducing a reaction.[1] After a time it was found that the shadow brought about a reaction much more frequently than at first. That is to say, something very like the conditioned reflex method was used. "It seems possible that the shadow and the touch have become so associated that the occurrence of the one intimates the probability that the other will follow. The response to touch is fairly uniform, and after a few such experiences the shadow, cast before, as it were, by the coming event more frequently produces a reaction."

This highly interesting experiment has apparently been

[1] Whether two different receptors are here employed is not certain.

confirmed for another species by Piéron (1929). "Molluscs and worms retreat into their shells or into their tubes when a shadow is thrown on them. If after the shadow a shock or a prick occurs, the reaction regularly takes place; but if nothing happens the reaction to the shadow quickly ceases to be produced. This is an acquired inhibition, conditioned in Pavlov's sense" (1929, p. 114).

It is, however, difficult to decide just what happened in these experiments. On the one hand it is possible that we have here a true instance of association, although had Mrs. Yerkes been writing today she would probably not have used quite the same terms to describe what she observed. That is to say, it may be that after simultaneous stimulation had taken place a few times the shadow stimulus served as the "signal" of the touch stimulus to come, which is the term used by Pavlov to describe the relation of the conditioned to the unconditioned stimulus. In this case it will have to be assumed with Mrs. Yerkes that the set of nervous impulses initiated in the tube-dwelling worms by the light were by association with those set up by touch diverted ultimately into the same motor paths. This is the view taken by Goldsmith, who claims that the Yerkes experiment is a true conditioned reflex (1927, p. 255). Yet this cannot immediately be assumed. For it has been seen that Mrs. Yerkes' experiment involved two kinds of stimulation which originally initiated neural impulses ultimately possessing the same final common path, while this is not the case with the usual form of conditioned response. In fact, while it is sometimes said that the conditioned response attaches a new response to a stimulus, here the process of "association" apparently reactivates a response that would otherwise have disappeared through repetition. In the one case a stimulus originally "inactive" with reference to a given response acquires active properties. In the other a stimulus is apparently prevented from becoming inactive by simultaneous presentation with another stimulus.

There are then apparently two possibilities, between which the experimental evidence hardly allows us to decide. The phenomenon may be a true conditioned reflex, a true act of association; or it may be an instance of dehabituation by extraneous stimulus, much as was discussed in the last chapter and illustrated on p. 141. It is at least certain that if a turtle were stimulated by a light tap, closely followed by a

violent one, and this were repeated, habituation to the less intense stimulation would require a much longer period; and it might be possible to account for this result without the association hypothesis by assuming that at each double stimulation habituation is progressively weakened by the succeeding violent stimulus. In this case it would seem natural to find that as the experiment proceeded and habituation began to set in to the violent stimulation the effect would diminish. This apparently occurred in the writer's experiments on the land snail (Humphrey, 1930). It may be seen to have occurred in the experiment on the turtle recorded on p. 141. As a matter of fact, the same effect is noted by Mrs. Yerkes. "The repetition of both stimuli many times in succession causes the effect to wear away temporarily, since the touch is not sufficiently injurious to force reaction" (p. 446, *loc. cit.*).

It is, however, conceivable that the two explanations by "association" and "dehabituation" really amount to the same thing. The phenomenon may be really a simple case of association accomplished through the process of dehabituation. First let us take the dehabituation experiment. Excitation of the turtle by a slight tap effects certain nervous elements. Excitation by a more violent blow affects a larger number of nervous elements, as may be seen from the fact that the animal now moves all its limbs and to a much greater extent than before. There is a mutual interrelation between the processes occurring in these two sets of neural elements such that after the more violent blow the "tap" now brings not habituation but excitation. This much we know without further assumption. But just the same statement is made by Beritoff concerning the mechanism of the conditioned response. He found, for example, that a preceding electrical stimulus favoured the conditioned motor reflex, where the primary stimulus was the shock, the secondary one a bell; and after ruling out several other possibilities for this result, came to the conclusion that it was due to the "irradiation of excitations from the cutaneous analyser (centre) into the focus excited by the sound of the electric bell." In fact, he claims that the conditioned reflex is determined by "the existence in the cortex of foci of raised excitability and of reciprocity between them owing to stimuli acting simultaneously on them" (Beritoff, 1924, p. 144). Here is an explanation for the

conditioned reflex which exactly fits Mrs. Yerkes' observations, and those of the writer and other workers on dehabituation. According to it, the intense stimulus and the habituation stimulus determine each of them in the nervous system foci of excitability which are mutually related, so that the habituation-stimulus, or more properly, in the terminology of this book, the negative-adaptation stimulus, now initiates reaction as the result of the general process of organic integration. This would make dehabituation, the Yerkes-Piéron phenomenon, and the conditioned reflex ultimately referable to the same fundamental formula. A paper of Sir Charles Sherrington seems to throw further light on this possibility.

According to Sherrington inhibition and excitation are due to the instigation in the reflex arc of processes or substances which have opposite effects. These he designates by the letters I and E. They are thought of as able to neutralize each other and as having their locus at the synapses. This assumption, though somewhat alien to previous notions of neural action, is of course supported by numerous observational data, and is able to unify a large body of experimental results which were hitherto isolated and unexplainable. But it is still an hypothesis, and is frankly so regarded by its eminent propounder. The evidence on which it was based is obtained from spinal phenomena, but there is no reason to believe that events occurring in the higher parts of the central nervous system are qualitatively different from those occurring in the lower. On the contrary, recent opinion seems to be inclined more and more towards the idea of an essential similarity.[1] Suppose then tentatively that habituation in such an experiment as Ada Yerkes' be considered as due to an accumulation of the inhibitory I. This supposition stands in line with the observed characteristics of certain habituatory processes. Habituation may disappear by lapse of time and it may be

[1] The following observation of Pavlov's may be quoted: "It is obvious that in its intrinsic nature our work is closely allied to the work of Sherrington and his co-workers upon the spinal cord, and it is impossible not to notice in how many points the different aspects of the nervous activity of the cortex correspond with those described in the physiology of the spinal cord; a fact which seems strong evidence of a similarity of the fundamental laws governing the nervous activity of the two cases." Pavlov, 1927, p. 378.

deepened below the zero point. That is to say, after it has proceeded to the point where a further stimulus produces no reaction, the process still continues, as may be seen from the fact that a longer or shorter recovery period may be necessary (Humphrey, 1930). These facts seem consistent with the hypothesis that habituation may be produced by the presence of Sherrington's I. Further, when habituation is established, a violent stimulus, by releasing a relatively large amount of E, the excitatory substance, might reasonably be expected to neutralize the accumulated I, and undo the work of habituation. That is to say, an intense stimulus may effect dehabituation, which has been seen to be the case. Finally, from general considerations it would seem reasonable that if repetition of a stimulus of slight intensity results in the accumulation of a certain quantity of I, then *repetition* of one of greater intensity would accumulate a larger amount of the same substance. This latter accumulation would prevent the occurrence of reaction to the less intense stimulus. That is to say, there would be habituation to the more intense stimulus, which would at the same time cause apparent habituation to the less intense stimulus. That is to say, the effect of the intense stimulus would appear to wear off. It has been observed that this is apparently what happened in Ada Yerkes' experiments and also in certain experiments of the writer's (1930) where repeated dropping of a metal ball gradually lost its dehabituating effect on the snail's withdrawal reaction. This tentative supposition would thus apparently fit many of the facts of habituation. It must, however, be said that there is considerable difficulty in imagining just how repetition of a stimulus could result in such accumulation, for at first sight it would appear that any inhibitory substance left between successive stimulations would be automatically neutralized by the next stimulus. So little, however, is known of the method of operation and production of these hypothetical agencies that such accumulation cannot be definitely set down as impossible, and it would not indeed be impossible to construct a scheme by which the necessary accumulation could be effected. Sherrington himself states that excitation may be connected with the production of the inhibitory substance. A further difficulty would appear to lie in the fact that the time intervals involved in Sherrington's data are much smaller than those involved in habituation.

The same plea of insufficient information may be made with reference to the problem thus created. The application of Sherrington's concept to the problem of habituation is undoubtedly attended by difficulties, but in spite of them such application seems justified as a very tentative hypothesis. Up to the present time no hypothesis appears to have been propounded.[1]

Passing to the somewhat more complex problem of the relation of the Yerkes experiment to association of the conditioned reflex type, it may now be stated that each seems to involve the existence of more or less permanent neural fields, though in the case of the worm these are probably much less clearly defined than in that of the higher animals. These two fields may apparently be thought of as influencing each other. In the Yerkes experiment the excitatory process spreads by "irradiation" from the one field, corresponding to the touch stimulus, to the other, which is the locus of those processes that are responsible for habituation. Here the effect of the E liberated by excitation would then be to neutralize the I, which is presumably responsible for the degeneration of response. Consequently, habituation would likewise be neutralized, and reaction should occur to the previously inoperative stimulus. Which is what is observed.

Thus, although the phenomena observed in the Yerkes type of experiment differ in important respects from those of the conditioned response, yet, as exhibiting the interaction of one field of neural action upon another, the two sets of phenomena are in certain respects closely allied. This is true, even though the Sherrington schema is not adopted. If this is accepted there seems reason for believing that it may provide a common explanation for habituation, Ada Yerkes' observations, and certain properties of the conditioned reflex. But it must again be insisted that we are here in the land of schemata and hypotheses. Those who venture on this terrain have often succeeded in making confusion worse confounded, especially when their steps are taken too confidently. The concluding sentence of Sir Charles Sherrington's paper may be quoted: "But the aim of such a provisional scheme can but be 'pour préciser les idées', so the further and the better

[1] The hypothesis would involve a similar mechanism for "extinction" and "disappearance" of secondary reaction in the conditioned response experiments.

to test them against fact." [1] And any such provisional schema seems better than the procedure so often adopted in psychological theory, by which an ill-understood process is canonized by an initial capital, entering as Adaptation or Attention a theoretical heaven towards which the investigator may only turn admiring eyes.

Ada Yerkes' experiment thus apparently stands as a *via media* between habituation, dehabituation and association proper. Habituation may be said to be a simple form of "modification by surrender", "learning not to do", change of response having proceeded not by the acquisition of a new response for a given context, as is primarily the case in the conditioned reflex, but by the dropping of one originally occurring in a given context. In the Yerkes experiment there is retention of response in spite of repetition, and because of the simultaneous presentation of another stimulus. There is retention, but not acquisition, while in the ordinary forms of the conditioned response proper there is both retention of response to the unconditioned stimulus and acquisition of new response for the conditioned stimulus.

[1] It is well here to add certain observations of Sherrington's which seem to argue, the first against, the rest for the possibility of the suggested application of his theory to the problem of habituation.

(1) "A stimulus which, when brief, is subliminal, becomes by prolongation, *e.g.* repetition, liminal or supra-liminal. 'Mere duration of the stimulus comes to be equivalent therefore to intensity'" (*loc. cit.*, p. 521). This seems to point to the *intensification* of excitatory process rather than to the possibility of habituation. It must, however, be remembered that the intervals of which he is speaking are much smaller than those used in the habituation experiments. In fact there is certain evidence that a stimulus repeated at too close intervals will be summated rather than habituated, or at least will not be followed by habituation. Something like this appeared to happen in certain of the writer's experiments with snails, where mechanical shocks at one-second intervals were not followed by habituation (Humphrey, 1930).

(2) The production of E and I, though it can take place independently may also be connected with each other. Thus "post inhibitory rebound contraction is a common sequel to strong inhibition." It is not therefore indicated that "excitation has no relation to or consequences for the production of I and *vice versa*" (*loc. cit.*, p. 531).

(3) By consideration of the effects of temperature (p. 543) Sherrington argues that his results tally with the idea that processes of such chemical nature are involved in reflex excitation and inhibition. It has lately been shown by Hoagland that learning in ants is dependent

Learning not to do.—Such "modification by surrender" appears in a more complicated form as "learning not to do" a thing. It has been seen that learning may be considered to consist in an adjustment to a total-stimulus. In "learning to do" the primary result from the point of view of the response is that reactions are retained, combined in a new way, or set off by a new stimulus. In learning not to do, the primary result is the falling away of response. In each case, there is established a constant state at the end of the learning process, but this constant state or equilibrium is in the one case static, in the other dynamic. A child learns not to touch burning objects. The final result of adjustment to the total series of stimulus-representations is in a normal adult indifference to the originally attractive stimulus. This indifference is a constant state of the organism with reference to the particular stimulus in question, parallel to the constant state achieved by habituation of all kinds. As involving lack of action, rather than action itself, such indifference is then static.

Now in its purest form habituation is typically unireceptoral, that is to say, as regards external stimulation. There are, however, certain cases where the same kind of statical equilibrium may be achieved by the co-operation of two receptors and two qualitatively different kinds of stimulation. These cases are such that a stimulus which was originally followed by overt reaction ultimately ceases so to be followed, not from repetition of the same stimulus alone, but because of the occurrence of a qualitatively different kind of stimulus, one affecting another receptor field. This process seems to be allied to the more simple habituation, although as involving two receptor fields it resembles also the more complex forms of association. It also may be treated as transitional to the

on temperature; the writer also found in a single set of unpublished experiments that habituation apparently depended in the tadpole on thermal conditions. This experiment was not confirmed, as at that time it appeared to have no bearing on the general problem under consideration. Lastly, Sherrington explains by his theory the fact that "increase of strength (within limits) of the stimulus applied to the afferent nerve shortens the initial latent period of the reflex." This may possibly have its counterpart in the increased latency observed in certain cases of habituation and "extinction."

It should be added that it seems exceedingly difficult to account for a lasting state of habituation by the application of Sherrington's concept.

latter type of reaction, again without any very definite implication of a strict line of demarcation.

In this category of learning by surrender come many of the learned avoidance reactions of early human life. This may be illustrated by the process of "learning to control the bladder".

The presence of fluid causes rhythmic contractions in the muscular walls of the bladder. These contractions stimulate sensory endings in the wall, the nervous impulses thus originated travelling to the spinal cord. Here, apparently by a process of summation, there are initiated impulses which produce motor impulses to the musculature of the bladder, and at the same time impulses which inhibit the contraction of the sphincter muscle which prevents the exit of the fluid. The whole process is quite automatic, and can be carried on without the co-operation of the higher centres, as was shown by certain cases in the war where injuries to the spinal cord severed the connection with the brain. In ordinary circumstances, however, this relatively autonomous block of reflex actions is under the control of the higher centres and ultimately of the cortex of the brain. This means that it can be brought into relation with the external conditions of the organism, as these are registered by the receptors. Thus at a very early age micturition may be caused to occur by stimulation of an auditory or other kind, and it may within limits be prevented from occurring except as a result of such stimulation. Such "control" is ordinarily attained in the period between birth and the fourth or fifth year. Its establishment has been shown, in the case of one infant, to follow the form of the curve of learning (Hull and Hull, 1919). The same process of training may be observed in a "house-clean" domestic animal. In both these cases it must be assumed that the stimulus for a whole block of reflex activity is present, but that operation of the whole mechanism is checked by inhibitory impulses from some point higher up in the nervous system. Whether such "training" is entirely due to something like a gradual process of "learning", or whether the maturation of nervous tracks also plays a part, it is impossible to say. But there is no doubt that in the adult civilized human being or house-clean domestic animal the primitive reflex is inhibitable by cerebral control. In its operation such control prevents the functioning of a reflex

whose biological stimulus is present. That is to say, considered from the outside it is an instance of "learning not to do", learning by surrender. Physiologically, however, the matter is not so simple, for the act of micturition involves, as we have seen, motor impulses to the muscles of the bladder and inhibition of the contraction of the sphincter. Control of micturition must therefore involve a double process involving the checking of both excitatory and inhibitory action, such process being possibly mediated through a common centre. In this complex process it appears certain that stimulation of the external receptors of the organism may play a part. Thus a baby is "encouraged" to gain control of the bladder, an animal is "punished" for relaxing such control, and so on. The process does not therefore involve the bladder receptors alone. It is "pluri-receptoral", at least under ordinary circumstances. Whether "cleanliness" in this sense could be acquired without the co-operation of the other sense organs it is difficult to say. Itard reports of his wild boy that cleanliness was only attained by making discomfort into a penalty. The boy had at first indescribably filthy habits, which were cured by "the certainty of passing the night in a cold wet bed" (Itard, 1894). The control of the bladder thus gives an instance of what has been called adjustment by surrender, where overt response is checked in a very complex way, not by the sheer repetition of the stimulus but by the co-operation of another receptor field. It will be observed that the same generic result is achieved as in the case where habituation is brought about by repeated stimulation of the same receptor field, namely failure to respond in spite of stimulation which might be expected to cause response. The difference is apparently in the number of receptor fields primarily concerned.

A similar example is given by the well-known experiments of Möbius (1873) [1] and Triplett (1901). Triplett kept perch in one half of an aquarium which was divided down the middle by a transparent glass partition. On the other side of the partition were minnows, at which the perch jumped but without success owing to collision with the glass. After a while the larger fish ceased to jump at the smaller, when the

[1] The writer has not had access to the original paper. The experiments have apparently been repeated, with interesting variations, in Pavlov's laboratory. See Ischlondsky, 1930, p. 179.

screen was removed and the minnows allowed to swim round the perch. This they did without molestation, the natural "reflex" having been successfully inhibited. This inhibition persisted except when the perch became hungry or when the minnows moved too fast. Triplett reports that after the partition was removed the perch would swim up to the place where it had been and then turn, as though it were still there, and that they apparently crossed into the other half of the aquarium only with hesitation.

Here again overt response to a stimulus was gradually surrendered, until the organism was left undisturbed by a stimulus which had originally caused vigorous reaction. With reference to this stimulus there has then been established a state of statical equilibrium, not, again, by sheer repetition of one stimulus but by the co-operation of at least two receptor fields. This shows again that just as habituation, which is the establishment of such a constant state by repeated stimulation, may be broken down by stimulation from another receptor field, so a process similar in its effect to habituation may be *induced* by such extraneous stimulation. The process is of great importance, and is ordinarily known as learning by punishment. In this way it is possible to inhibit even the most powerful responses, such as that of food taking. Certain frogs, for example, with which Schaeffer experimented refrained from eating hairy caterpillars, and did not feed at all for some days after being "punished" with an electric shock for eating certain kinds of food (1911). The reaction of sex may be checked in a similar way, and up to a certain point (Moss, 1924). In fact, many features of our civilization are apparently only made possible by the "social inhibitions" which are highly complex instances of "learning by surrender" and have their simple counterpart in such experiments as Triplett's. The decalogue, couched as it is mainly in negative terms, is a comprehensive statement of the chief situations where a primitive society erected such social inhibitions. The Freudian doctrine of the Censor refers to the network of social situations where civilization has decreed overt inactivity in place of the activity that would otherwise follow. As contrasted with the simpler experimental instances in which animals were used these human "inhibitions" are of course of almost incredible complexity, and great caution must be exercised before arguing from one to the other. But the fundamental effect of

experience seems to be similar in both cases. An organism is originally excitable to a certain course of action by a fairly definite stimulus. In the case of the animals such excitation was directly observed, in the case of the human being our experience of human nature and that of generations of law-givers enables us to deduce with some certainty that it would occur under "natural", untrained or uncivilized conditions. By means of a further excitation of other sense organs, which in the human examples is of a high degree of complexity, such excitation is checked. We do not "learn" not to kill our fellow-citizens by killing a large number of them and thus gradually becoming indifferent to their presence. Stimulation of another quality is necessary to teach this lesson, and this further stimulation is effected by our total general and moral training. At least, this is true of such of us as are not criminals. Thus we actually do acquire to a stimulus an indifference which, in effect, is similar to the indifference resulting from habituation, but which differs from it in that it is brought about by stimulation extraneous to that presented in the situation concerned. If we "spare the rod" we are said to "spoil the child".

It is then apparently possible to make two classifications of stimuli according as their effects are inhibitory or excitatory, and further as these effects are innate or individually acquired,[1] giving in all four main classes. There is the stimulus which produces an overt reaction in the living system, the first time, let us say, it is presented. Such are the stimuli for the positive reflexes such as Sherrington treats, and such, presumably, as the auditory stimulus producing a blink in a baby a few hours old. There is the stimulus which produces such an overt reaction not "originally" but because learning has taken place. In the terms of this book, such a stimulus produces the response in question only when it occurs at a term later than the first of an action series. Such are the conditioned stimuli of Pavlov, and the situations involved in most of the vastly more complex acts of human learning. In the same way, there is the "innate" inhibitory stimulus, such again as Sherrington treats in his study of the more primitive

[1] This classification is, of course, of pragmatic value only. It is immaterial in this context whether or not all reactions of any kind should be considered to be acquired, that is to say, as due to the effect of individual environment. See Carmichael, 1925.

reflexes, and such, probably as the intense stimuli which are the objective conditions of "attention". Examples of inhibition innately determined are to be found plentifully in the internal economy of the body, as in the reciprocal inhibition of antagonistic muscles, though here the same stimulus is, of course, a positive determiner of action for the opposed muscle. Finally there is the acquired inhibitory stimulus, which is again simply illustrated by many experiments of Pavlov, and is shown in a more complex form in the cases discussed in this chapter. In Thorndike's terminology, there are bonds which may be either innate or acquired, and either leading to action or inhibiting it. As resulting in a state of inactivity, similar to that which is produced in many cases by sheer repetition of the stimulus, and as being in that respect similar to the simpler habituation, the latter class of stimulus-response relation has been treated in this chapter before the conditioned reflex proper; although as requiring the co-operation of at least two receptor fields these reactions might equally reasonably be treated as a special case of associatory response.

CHAPTER VIII

THE PROBLEM OF ASSOCIATION

" Mortimer revenait tremblant d'un long voyage ; il adorait Jenny. . . . Elle se promenait dans le parc ; il y court, le cœur palpitant . . . il voit qu'il est aimé . . . la robe de Jenny s'embarrassa dans un buisson d'acacia épineux. Jenny fut infidèle . . ., jamais il n'a pu me donner le moindre détail. Seulement il tresaille visiblement dès qu'il voit un buisson d'acacia." (Stendhal.)

It has been seen that from an action series of the simple type where an organism is subjected to a rhythmic series of temporally discrete stimuli affecting one receptor only there may schematically be developed a simple type of association by adding at each such stimulation a further stimulus affecting a second receptor. Thus is obtained a response of the conditioned reflex type. It is time to consider this type of reaction in detail.

Historical Development of Conditioned Reflex Concept.—The term conditioned reflex is associated with the name of Pavlov, the eminent Russian physiologist, in 1904 Nobel Prize-winner for his work on the digestive glands. According to his own account Pavlov's investigations were initiated by the observation that so-called " psychic " factors played an important part in the process of digestion. The resulting series of papers on the conditioned reflex has lasted continuously for over thirty years, the first being published in 1899 under the title " Observations upon Salivary Secretion " (Pavlov, 1927, p. 412), while the last, it may be hoped, is yet to come. The study of the salivary secretion was not new. The secretion of the salivary glands had been investigated by many workers under the name " psychic secretion ". What was new was the method whereby the apparently capricious action of these glands was brought under the rubric of exact science without the intervention of a psychic principle to act as a physiological *deus ex machina.*[1]

[1] Pavlov, 1926, p. 8, Lecture delivered in 1903.

In giving the world the conception of a reflex as the necessary action of an organism consequent upon stimulation of a receptor, Descartes had introduced into physiology the ideal of a uniformity parallel to that of the inorganic sciences. According to this conception, reflex movement followed the appropriate stimulus necessarily and unconditionally by the unvarying sequence characteristic of natural law.[1] Contrasted with such movement seemed to be those organic actions in which some principle other than inorganic cause and effect seemed to participate, actions where some such factor as " choice " was apparently operative. To express it otherwise : when food is placed in the mouth of an animal, reflex action produces salivation. Salivation can also be produced, however, in other ways, such as by the sight of the food, or even by the sight of a person who usually brings food. For such action there is no direct physiological connection between receptor and gland, and to explain it investigators had postulated a psychic factor. Men assumed, for example, that the mind of the dog makes a connection between the food and the person who brings it, the process being parallel to the association of ideas which was thought to explain similar actions in human beings. Other actions seemed to necessitate some power of choice on the dog's part. The dog may or may not salivate when he hears a bell, he may or may not come when his name is called. The fundamental principles of physical science seemed here to be negated ; such phenomena were and often still are declared to be outside the range of scientific method.

Pavlov's attack on this elusive problem was highly ingenious and simple—so simple indeed that its full importance is difficult to appreciate today. It consisted in showing that the action of the salivary glands only appeared to be capricious because all the relevant circumstances were not taken into consideration. Whether or not any particular dog salivated in any particular situation depended upon the fulfilment of certain strictly material conditions. If these conditions were fulfilled Pavlov showed that such secretion could be just as closely controlled and prophecy just as accurately made concerning them as in the case of any other reflex. Thus it was found

[1] Descartes, *Traité de l'homme*, 4, pp. 130 ff. Descartes compared the action of a stimulus to that of the foot in setting off certain mechanical figures. See Skinner (1931) for an estimate of Descartes' contribution.

possible to dispense with the hypothesis of a psychic principle and to apply the standard methods of physical science to a class of problems that had hitherto seemed intractable and mysterious. It is worth while to quote Pavlov's own words, written at the beginning of the experimental period in 1904. "There now arises an extraordinarily important question: by what means would it be possible for us to investigate these latter psychic relations? After we had employed a number of different methods, we determined to use the objective method in the investigation of this matter also. This means that the experimenter ignores the conceivable subjective state of the object of experiment, and concentrates the whole of his attention upon the problem of ascertaining in fullest possible detail the external conditions which have any influence upon the working of the salivary glands. The point of departure for such investigation was formed by the idea that so-called psychic secretion was fundamentally a specific reflex, of the same nature as secretion initiated by stimulation of the oral cavity, with the difference that this psychic reflex may be initiated by stimuli affecting other receptor surfaces, and that it is a temporary reflex, one dependent upon determinate conditions. In this way the goal of our further inquiries was clear: it consisted in the determination and investigation of those conditions under which these special reflexes appear." [1] By contrast with the unconditional reflexes of Descartes these reactions were termed "conditioned" reflexes.

It was soon found that the ordinary laboratory precautions of the time were inadequate for the determination of these excessively delicate conditions. The point is well made by Pavlov in another connection. "Constantly, every minute one might say during our experiments, there may be seen a positive reaction of the animal to each variation of the surrounding situation. Every sound, however faint, that chances to occur, amid the constant sounds and noises that surround the animal, every weakening or increase of these constant noises, every oscillation of intensity of the general illumination of the room, whether the sun hides behind the clouds or a ray of light crosses them, every sudden raising or lowering of light of a little electric lamp, a shadow which crosses the window or the room, a fresh odour that spreads

[1] Pavlov's Nobel Lecture, Stockholm, 12th December 1904 (Pavlov, 1926, pp. 24, 25).

in the room, a puff of cold or warm air that penetrates it, even the lightest possible grazing of the animal's skin, such as a fly, a fragment of plaster from the ceiling, etc.", may excite the activity of this or that part of the animal and interfere with the experiment. So delicately attuned is the complex organic system to external changes (Pavlov, 1913). In 1910, therefore, by the generosity of a business man, there was built a remarkable new laboratory in which extraordinary precautions were taken against adventitious disturbance. Some idea may be given of the thoroughness with which this work was done from the description given of the sound-proof rooms, the walls of which comprised successive layers consisting of wax-cloth, two layers of wadding, cement-embedded metallic net, cork, ashes, twisted hemp, two layers of felt, veneer, sawdust, veneer, two layers of felt, hemp, air-space, ashes, tar, two layers of felt, cement-embedded metal net, two layers of wadding, and wax-cloth! The doors of these portentously isolated rooms were double, and consisted in all of thirty-six insulating sheets. These elaborate rooms were as a matter of fact found unnecessary for routine work and were reserved for the more delicate investigations. According to the technique gradually developed the operator was not present in the same room with the dog, the salivary secretion being automatically recorded by an ingenious pneumatic apparatus. Elaborate measures were used also to ensure constancy and control of the stimuli.[1] Further details of the methods used will be discussed later.

While Pavlov was developing his method, other workers were active along similar lines. Thus the late Professor Bechterew, Pavlov's rival savant, worked with human subjects on what he called the "associative reflex"[2] at St. Petersburg, using mainly the breathing reflex and the motor reflex produced by giving the subject an electric shock such that the leg was withdrawn. Kalischer in Germany used the same method on dogs.[3] There seems to have arisen between these three workers an unfortunate question of priority. Anyone who will compare the publications of the three must, however, come to the conclusion that in quality and quantity of output, and in general power of thought, insight into the problems

[1] See Podkopaew, 1926, for a complete description of technique.

[2] Bechterew, 1913, 1926.

[3] Kalischer, 1907, 1909.

and ingenuity in overcoming the severe difficulties involved, Pavlov is overwhelmingly superior, even though historical accident of the general trend of ideas may have inspired other investigators to work simultaneously or even slightly before him along the same lines.[1]

The possibilities of the conception were soon appreciated by the behaviourist school of psychology in America. The Russian objective school initiated by Setschenow in 1866 had indeed much in common with the vigorously combative American movement. In 1912 Watson published a paper setting forth the possibilities of the new method, and the conditioned reflex has thenceforward become one of the pillars of behaviourist thought. Since that time there have been made along the lines struck out by Pavlov many experiments, not only on dogs and human beings but on forms of widely differing degrees of complexity. Among these are snails, fish, various marine animals such as crabs, squids, and the clam-worm, turtles, birds such as hens and pigeons, cats, guinea-pigs, sheep and monkeys; in addition, conditioned reflexes have been obtained upon children by various methods, and on various human structures, such as the leg, already mentioned, the finger, the eyelid and pupil, the knee-jerk, the salivary reflex, and lastly, the so-called "psychogalvanic reflex".[2]

[1] See Pavlov, 1926, p. 4 *n.*, for what appears to be an entirely justified rebuke. Pavlov was not the first to treat the "psychic processes" as reflexes; his priority lay in putting a rather vague conception into strictly controllable scientific form. See the interesting note in Bechterew, 1926, p. 153. The history of the Russian school of objective psychology is taken up in Kostyleff, 1914.

[2] The references are as follows: Thompson, 1917, Garth, 1926, snails; Froloff, 1925, 1928, fishes; Mikhailoff, 1920, 1920*a*, 1921, 1922, 1923, crabs and the squid *Eledone Moschata* (Mikhailoff is apparently a pupil of Bechterew); Bechterew (1926, p. 314 *n.*) refers to conditioned reflex work on the axolotl; Copeland, 1930 (one clam-worm); Parschin, 1929, turtles; Watson, 1916, hens; Ten Cate, J., 1923, Beritoff, J., 1926, pigeons; Upton, 1929, guinea-pigs; Wever, 1930, cats; Liddell, 1927, sheep; the official (Sovkino) film exhibiting Pavlov's results shows an (imputed) conditioned reflex in a monkey; Krasnogorski, 1909, 1913, Mateer, 1918, Smolensky, 1927, children; Cason, 1922 and 1922*a*, pupil and eyelid; Lashley, 1916, Winsor, human salivary reflex; Schlosberg, 1928, knee-jerk; Freeman, 1930, Jones, 1931 (with references), Slight, 1929, psychogalvanic reflex. To these should be added the vaso-motor reaction and the general motor reaction described by Ischlondsky (1930, pp. 42 ff.). He refers also to

The general scheme of the experiments is now sufficiently well known. In the original work of Pavlov it consists essentially in subjecting the animal to some form of biologically adequate stimulus for salivation, such as food in the mouth, together with a second stimulus, such as light, which was originally without effect on the salivary reaction. As is well known, the second stimulus comes to call out the salivary reaction. In Pavlov's terminology it becomes a stimulus for this response if the condition has been fulfilled that the two stimuli have been presented together sufficiently often. In addition to the original term "conditioned" such stimuli as the light are often called "active", as contrasted with "inactive" stimuli which after training have no effect on the response in question; sometimes they are termed "secondary" by contrast with the "primary" or biologically adequate stimuli.

An integral part of the method is objective registration of response. In order to make this possible the dog is subjected to a slight operation which, according to all accounts, leaves him in perfectly normal health. The skin is cut and a small piece of mucous membrane inside the mouth and containing the opening of the canal from the gland is turned through to the outside. The secretion [1] is thus conducted to the outside of the animal's cheek and flows drop by drop into a specially designed recorder. Seven or eight days after the operation the dog is placed in the experimental chamber, stroked, called by his name and fed a small portion of meat powder by hand. On the next day he is put in the special stall and attached to it by a collar only. On the same day he is fed by hand and by the special feeding apparatus. After ten or fifteen minutes he is freed and given a bone. Henceforward the object is to accustom him gradually to the apparatus. On the third day the door of the experimental chamber is closed; on the fifth the animal is fed four of five times with the sound of a metronome. The experiment has begun.

The time of appearance and the fixation of the conditioned

respiratory reflexes. The list of course makes no pretence at being exhaustive. Its purpose is merely to show the extent of the conditioned reflex work. A bibliography will be found in Cason, 1925.

[1] Either the parotid gland in the cheek or the submaxillary or sublingual gland under the tongue.

response varies according to the receptor surface employed, the precise method used, the individuality of the dog, and other such factors. In general, indications of "association" appear after from three to five simultaneous presentations, fixation requiring approximately ten to fifteen simultaneous presentations.[1] Pavlov gives twenty repetitions as ordinarily necessary before the conditioned stimulus will uniformly evoke the required reaction in its full strength.[2]

Pavlov's Hierarchy of Reflexes.—If this apparently simple, because familiar, procedure is analysed, a host of intricate problems is raised. For a full discussion of these the reader is referred to Pavlov's own account of his work, which has been translated into English under the title *Conditioned Reflexes* (1927). Certain questions only will here be canvassed on account of their particular relevance to the conception of the organism as a living system in intimate relation with environmental conditions. Perhaps the most obvious problem concerns the difference in the effect of the two repeated stimuli. Such difference has already been recognized in the chapter on habituation.

When two stimuli repeatedly fall simultaneously upon the receptor surfaces of the same organism, what decides which way the conditioning process will go? According to what principle will the bell ultimately cause the animal to salivate, while the food does not cause him to look to the bell? Pavlov answers this problem by assuming that there is a certain hierarchy of reflexes. The unconditioned reflex must be physiologically the stronger and of biologically greater importance. The alimentary reflex stands very high in the hierarchy although there are reflexes such as those involved in self-preservation which stand higher.[3] Even the nervous impulses causing a fixed and lifelong response can be diverted to another more powerful centre. What causes a "powerful" reflex Pavlov does not say.

The idea of a hierarchy of reflexes is very similar to Sherrington's conception of the prepotent reflex which dominates in competition with others for the final common path.[4] Pavlov's hierarchy, however, exhibits variations apparently lacking in Sherrington's scheme. It fluctuates with the fluctuating effect of certain biologically adequate stimuli.

[1] Podkopaew, 1926, pp. 31, 35.
[2] 1927, p. 31.
[3] 1927, p. 385.
[4] Sherrington, 1911, p. 228.

In order to provide energy for its behaviour, and thus to perpetuate its own existence, the organism must from time to time replenish its supplies of certain materials, such as the energy producing and structure building substances called food, the equally indispensable liquid that serves for drink, oxygen and so on. Now in the higher organisms the " needs " corresponding to these replenishable materials occur more or less rhythmically. Thus man takes his food at " meal-times ". Among animals in natural surroundings movement towards water occurs at intervals during the day, depending on circumstances. Experimental observations have been made upon the periodicity of such food- and drink-movements. Among certain animals they have been observed to occur at surprisingly regular intervals, almost paralleling the " meal-time " of the civilized adult. " The records show that the rat drinks about ten times a day at intervals of two and a quarter hours. The periodicity of the thirst response is quite as remarkable as that of the hunger response, in view of the current conception that the rats eat and drink at very frequent and irregular " intervals ".[1] Thus stimuli corresponding to these fundamental " drives " are weighted from time to time and, in many forms of life, rhythmically. This weighting of special stimuli is apparently only an accentuation of the generally increased sensitivity known to occur when the animal is " hungry " or " thirsty ". A hungry animal is " restless " in general ; the threshold for a large number of stimuli is lowered, but the animal is apparently particularly responsive to food. When food has been taken, this special sensitivity seems to disappear, and the conditioned reflex is not formed. Thus Warner notes that if animals are let into the food compartment after they have just eaten they " fail to respond to the food any more than to other details ".[2] Accordingly it is an obvious laboratory rule that when the food-reaction is being used to form a conditioned reflex, experiments can only continue until the animal is satisfied, the daily number of feedings varying from animal to animal, with an average of from eight to ten. When acid is used not more than five or six applications are possible. With the electric shock stimuli may be repeated for an apparently unlimited number of times.[3] In the same way the female

[1] Richter, 1927.

[2] 1928 (Warden, 1931, p. 61).

[3] Podkopaew, 1926, p. 32.

sex reaction shows a periodicity, at least in the mammal, though Warner [1] does not detect any similar phenomenon in the male. The duration of these periods is, of course, different in the case of different urges, that for oxygen being considerably shorter than the others considered. These primary needs, which depend ultimately upon the fact that the organism is a self-maintaining system with its own energy reserves, correspond to what with others are called by Woodworth dependable motives.[2] They are, indeed, dependable but only on occasion.

It is apparently on this fluctuating difference of sensitivity that Pavlov's hierarchy depends. The hierarchy functions only at specified times. It is, in the life of the animals, a rhythmical phenomenon. At certain times it is overthrown. The conditioned response may be properly described as motivated, and as subject to the fluctuations inseparable from the particular motive that is used. The fluctuations depend on the fact that the equilibrium of the organism may be disturbed not only from without, but also from within, stimuli corresponding to organic " needs ", which are of a fluctuating character, themselves exhibiting a corresponding fluctuation of effect. Stimuli calling out protective or conservative responses are, apparently, relatively invariable in their effect, at least when the response in question is intense.[3]

Closely connected with the problem of why the bell causes the food reaction, rather than vice versa, if indeed it does not involve the same fundamental problem, is the fact that the responses of the two stimuli behave differently on repetition. Attention has already been called to the fact that under normal conditions the secondary stimulus is originally followed by response equally with the primary one. " It must be

[1] In Warden, 1931, pp. 119, 120.

[2] 1929, p. 246.

[3] The conception of the hierarchy of stimuli or reflexes (Pavlov, 1927, with author's preface written in 1926) seems now to have been supplemented by that of the dominant (Ischlondsky, 1930), whereby the direction of the central impulse initiated by an external stimulus is determined by the momentarily " dominant " centre. See Pavlov, *op. cit.*, p. 31, and Ischlondsky, pp. 213 ff. " Dominance " would then in part be rhythmically determined by periodic organic needs. It should be added that Pavlov himself states that the unconditioned stimulus depends on the fulfilment of definite, though fewer, conditions (1927, p. 24).

remembered that there are no absolutely indifferent agents for the dog; every agent used for the first time calls out an orientation reaction, which varies according to the strength of the agent in question and the individuality of the animal. The agent chosen as conditioned excitant must, however, be thoroughly indifferent with reference to the unconditioned reflex." [1] After training this "orientation" reaction disappears. Thus Pavlov states that in one of his public experiments, "In this very experiment the dog turns in the direction from which it has been customary to present the food and begins to lick its lips vigorously. . . . The sound of the metronome is the signal for food and the animal reacts to the signal in the same way as if it were food; no distinction can be observed between the effects produced on the animal by the beating metronome and showing it real food." [2] The original "orientation" response to the bell has gone, and its place is taken by reaction towards food.

What we have in fact is the repeated simultaneous presentation of two stimuli, the responses to one of which (salivation) persists, while that to the other (orientation) disappears. Now it must be remembered that under the proper conditions response to the food stimulus will disappear equally with that to the light, and that these are the conditions under which the "association" does not take place. Thus it has been seen that the satiated animal in whom food responses have disappeared cannot be "conditioned" to food. It is not then an absolute persistence of the unconditioned response with which we have to deal, but rather one which depends on circumstances and one which contrasts with the relative ease with which secondary response degenerates. As the experiment is performed in the laboratory the response to the conditioned stimulus does fall away, while under identical conditions of repetition that to the unconditioned stimulus remains. Further, permanent indifference can easily develop to the sound of a bell if this is repeated at the proper intervals, while permanent indifference to food can never be developed in a living creature. Thus a flock of sheep by the railway line will rapidly grow indifferent to the passing trains but can never grow permanently indifferent to the water in the stream. We thus have a problem of what may be called "differential persistence" of reaction.

[1] Podkopaew, p. 8.

[2] 1927, p. 22.

At first sight it seems that we have here to do with the phenomenon of habituation. It will be remembered that it is the intense nocuous stimulus to which habituation is with difficulty, if at all, achieved. In the ordinary conditioned reflex experiments arrangements are specially made to the end that habituation or its counterpart, satiety, does not take place to the alimentary stimulus. Thus it might be thought that conditions in these experiments are such that because of a difference in speed of habituation there naturally follows a difference in the persistence of reaction to the two stimuli used. For one of the stimuli has been specially selected, either in time of application or in quality, in order that its response may persist while the other response dies down. As long as response to the conditioned stimulus degenerates more quickly than that to the unconditioned stimulus, at the end of the experiment we must then be left with response to the latter alone. Many experiments are so arranged that the conditioned response never degenerates under the given conditions. This is not necessary, however; all that is needed on this view is a difference in habituation time. Such an hypothesis would explain the action of the stimuli in association by their intrinsic action apart from the fact of association.

There is a certain amount of evidence for this view, which would regard the experiment as involving a kind of race for the extinction of response. If adopted, it would mean that the effect of repetition upon response to the conditioned stimulus is the same—namely, habituation—whether a conditioned reflex is being formed or not. There are, in fact, three phenomena which exhibit very similar characteristics, namely, the extinction of the original response to the conditioned stimulus when given in isolation, extinction of the same response during the formation of a conditioned reflex, and extinction of the fully formed conditioned reflex.

The last of these phenomena, namely, that termed by Pavlov the extinction of the conditioned response, here needs a certain preliminary description. It is well known that if a conditioned stimulus such as a light is repeated a number of times without the food, its power to produce the conditioned reaction gradually falls off, until it is finally again inactive. Such extinction has often been observed in human subjects. Thus the writer trained a subject to respond to

a certain musical tone by raising his hand, the primary stimulus being an electric shock. When the training was complete, two "conditioned" stimuli at five-second intervals were each followed by responses, but a series of twenty-five, beginning with a pair at quarter-second intervals, produced only one such response. Temporary "extinction" had taken place, which was removed by a rest of half a minute. Such "extinction" has been observed by Pavlov to take place, with increasing latent period, in conditioned responses established on dogs (1927, p. 49) and by Bechterew, who worked with human beings, and also notes a longer latency as the extinction progresses (1926, p. 244).[1] The phenomenon has always been observed when it has been looked for, and is in fact a necessary corollary to the formation of the conditioned reflex; an association that, once learned, could not be unlearned, would be a biological absurdity, and repetition of the light without the food is clearly the situation above all others that should lead to unlearning.

Now Pavlov found that the extinguished reflex could be restored by extraneous stimuli,[2] by the unconditioned stimulus, or by lapse of time. It has been seen that similar methods may be used to restore responses that have disappeared by habituation. Many authors have reported dehabituation by lapse of time, several have observed its occurrence as the result of another stimulus. As already pointed out, the latter phenomenon may easily be exhibited in the human wink reaction, which may be restored by a rather more intense stimulus after disappearance through repetition. It has been observed also by Holmes in sea urchins (1912), by the writer in snails and turtles, by the writer and his colleague, Dr. G. S. Melvin, in frogs. Similar experiments are reported by Piéron, who draws the parallel with Pavlov's disinhibition, and by Pavlov himself (1927, p. 256). Pavlov then claims

[1] Cf. Beritoff, 1927, p. 142.

[2] It is noteworthy that Pavlov found that very intense stimuli were less efficacious than weak ones in removing extinction. The cause seems in his opinion to lie in some obscure relation between the excitatory and inhibitory processes. The writer used rather intense stimulation to restore reaction, as in fact did Pavlov when he restored the response by means of acid which caused a flow of saliva lasting over some minutes. It is probable also that, as in Pavlov's experiments, the intense stimulation used by the writer induced in the structures concerned a reaction of a different systematisation.

that the two processes, "extinction" of the orientation response, which is what we have called habituation, and extinction of the conditioned response, are the same, referring, for example, to an investigation by Professor W. A. Popoff,[1] which, he says, proves their identity. In addition there must be added the weighty authority of Beritoff (1927) who makes much the same point.

Apparently it is nowhere explicitly stated that the same process is also involved in the disappearance of the investigatory reflex, during the formation of a conditioned response. Pavlov does state that every new stimulus which invokes the investigatory reflex ceases on repetition to have any effect unless the stimulus has been followed up with some other reflex.[2] This would perhaps indicate that in respect of disappearance of response he makes no distinction between the action of such a stimulus when presented alone or simultaneously with the conditioned stimulus.

In fact, there is no doubt that we here have to do with three very similar processes: that of habituation, which involves the fading of response when an isolated stimulus is repeated while keeping all other conditions constant; that of the disappearance of reaction to the secondary stimulus of a conditioned reflex, which occurs when the two stimuli affecting different receptor fields are simultaneously repeated; and that of extinction, which occurs when a stimulus already conditioned is repeated without the primary stimulus—the bell without the food. Many instances of "habituation" as discussed in Chapter VI could have been predicted from Pavlov's results. These instances are independently observed to have the properties of Pavlov's extinction. At least it should be said then that in the present state of knowledge it appears to be the part of caution to assume that a common mechanism is involved in "negative adaptation", "extinction of the conditioned response", and the disappearance of the original response to the conditioned stimulus.

Yet it would be a mistake to assume that the three phenomena should therefore be identically equated. For this would mean, for example, that the process of negative adaptation is unaffected by the fact that a unifying process

[1] Pavlov, 1926, p. 288. I have not had access to Popoff's paper. Cf. Rosenthal, 1929.

[2] 1927, p. 385.

of "association" is simultaneously taking place in the brain, an association which brings it about that nervous impulses originating in the one receptor ultimately come to discharge along paths leading to a hitherto indifferent effector. It is unthinkable that such a process of unification should occur without at the same time exercising any effect upon the cerebral changes initiated by the first receptor. It is impossible, for example, to believe that the cerebral changes initiated by the repeated ringing of a bell should be unaffected by the fact that there is simultaneously occurring a cerebral synthesis such that the bell will ultimately cause a "food" reaction. If negative adaptation is taking place in the first case, something more is occurring in the second. To believe otherwise would involve a mental atomism entirely contradictory to all we know of the unitary character of cerebral function. Similarly degeneration of response cannot be mediated by identical neural processes during extinction of a conditioned response and straight habituation. In fact, from general principles it is clear that the neural processes initiated by the two stimuli of a conditioned response must have a mutual effect on each other. This indeed is precisely the conclusion to which Beritoff came as a result of his experiments on the conditioned motor reflex in dogs. He found that when a stimulus is applied to another receptor field simultaneously with the electrical excitation of the hind leg, there is a reciprocal action between the cerebral centres concerned,[1] the development of the reciprocal relation proceeding *pari passu* with the formation of the conditioned response.

A study of Pavlov's results leads to the same conclusion (1927, lecture 14). Negative adaptation appears to be simple, while the disappearance of the unconditioned response and the extinction of the conditioned reflex would seem each of them to involve a factor additional to the simple, habituatory process. After all, when a conditioned reflex is extinguished not only is the bell, let us say, being repeated, but the accustomed food is being withheld. Kleitman and Crisler (1927) indeed, by a mathematical analysis of the process of salivary extinction in dogs, found evidence that a double process was involved, though their examination did not show a corresponding double process in the formation of the conditioned reflex. They add a specific warning that the

[1] Beritoff, 1924, p. 129.

data were not such as to allow more than a plausible conjecture here to be drawn from their results.

Most illuminating of all in this connection is perhaps the excellent monograph of Wendt on the conditioned knee-jerk. Wendt (1930) employed the ingenious method of making a stimulus on one knee into a conditioned stimulus for a reaction on the other. The left and right legs were successively stimulated at an interval of one-fifth of a second, the right stimulus being ultimately omitted. Thus the experiment began with two clearly marked responses, to the left and to the right legs; but as it progressed response to the left leg diminished while at the same time stimulation of the same leg comes to excite a response in the other. Here is a case of the disappearance of the original response to the secondary stimulus, entirely parallel to the disappearance of the orientation response. The particular interest of the experiment comes, however, from the fact that under comparable conditions the reaction employed, namely the knee-jerk, does not change in amplitude as the result of isolated repetition.[1] Here, then, the disappearance of the original response to the secondary stimulus must be due to processes incident to the formation of the conditioned reflex, not to a habituatory process initiated by sheer repetition of the secondary stimulus itself.

Thus when the conditioned stimulus is repeated during the formation of a conditioned response, there are probably initiated processes of two kinds, namely, those that result from sheer repefition, corresponding to what has been called habituation, and those that come from the simultaneous action of the two stimuli. When, again, the same unconditioned stimulus is repeated alone after the conditioned reflex has been formed, that is to say, during extinction, there must occur processes corresponding to the dissolution of the reciprocal, connection-forming relations that come into being when the conditioned reflex is being established. So that here again we would expect to find the effects of sheer repetition complicated by the fact that repetition is taking place in connection with a conditioned response.

These results make it entirely impossible to believe that there is nothing more involved in "extinction" or in the

[1] "In the case of subject Mon. . . . no change in the amplitude of response was observed as a result of 100 single stimulations on each of three successive days" (Wendt, pp. 69, 70).

disappearance of secondary response than the process whereby the same stimulus gradually loses its exciting effect when repeated in isolation. Likewise they render impossible the hypothesis that, in the persistence of the one response and the disappearance of the other during the formation of the conditioned reflex, we have to do with nothing more than a race for habituation. However, it would, of course, be unjustifiable to assume, *per contra*, that in the three processes above considered, we have to do with three fundamentally different mechanisms. In view of the similarities discovered between them and the weighty authority of such independent neurologists as Beritoff, such an assumption would, in the present state of knowledge, be gratuitous and confusional.

The relation existing between the three phenomena just considered affords an excellent illustration of the dangers lurking in a premature simplification of the complex problem of learning. The process underlying habituation very probably takes part in the more complicated process of " extinction ". Yet the latter cannot be explained by the former. Considered in relation to extinction, habituation may be regarded as a limiting case where the influence of the second stimulus is zero. We cannot tell without introducing new considerations what effect the incidence of a second stimulus or change of conditions affecting another receptor will have upon a response which is disappearing through habituation. Still less can we assume that this effect is zero. Such a mistake may be called the " fallacy of the limiting case ". It leads to the misleading procedure whereby a case is examined because of its simplicity, and the more complex instance is thereupon assumed simultaneously to have been explained. Contrasted with this false procedure is the legitimate method of simplification which consists in keeping constant all but one of the factors affecting the complex case, in order to examine the rôle of the one selected factor. The latter procedure has played and will play a valuable and necessary part in psychological experiment. To the former we owe, as we shall later see, certain seductive, somewhat spectacular, but very misleading generalizations.

To explain the disappearance of the original response to the unconditioned stimulus an alternative hypothesis has been proposed according to which drainage of neural energy takes part ; but it is hard to see how this hypothesis could

account for "extinction". In any case the root hypothesis of neural drainage is difficult to accept. Whether again the newly developed doctrine of the "dominant" can here be invoked it is as yet hardly possible to say. On the whole it appears at present to be the part of wisdom to regard both these phenomena of response-degeneration in the conditioned reflex as involving some factor or factors which we cannot as yet classify.

Thus, exactly as in the maze experiments to be discussed later, an important part of the problem of the conditioned reflex concerns the disappearance of response, this being perhaps equally important with the more obvious "associative" aspects.

Generalization and Differentiation.—When the conditioned reflex is first established certain characteristic phenomena seem to be universally observed by those who look for them. As important phenomena of learning these will be discussed in some detail. There is first what is called "generalization". Not only does the conditioned stimulus now set off the primary reaction; it shares this property with a wide range of allied stimuli. Thus in certain experiments human subjects were given the note A natural on a metal instrument of the xylophone type; at the same time they received an electric shock through the right hand. After a number of such repetitions the sound of the note alone came to call out withdrawal of the hand.[1] When this took place, it was found that any of the twelve notes on the instrument had the same result, although they had never been given at the same time as the shock. Further, slight noises inside or outside the laboratory were found to be equally effective. Thus in the early stages the slam of a door in another part of the building would occasionally cause movement of the hand, especially in a "nervous" subject. I have often been able to elicit the response, for demonstrational purposer, by shouting from behind the operator's screen. Any environmental change may, in fact, have the same effect, and Pavlov reports that the whole environmental situation may acquire at first conditioned properties. Pavlov, who, of course, was the first to describe the phenomenon, reports generalization of musical tones, tactile stimuli, and those affecting other receptors. What are called trace ("Spur") reflexes are reported to be particularly subject to

[1] Humphrey, G., 1927.

generalization, which here extends over all the receptors and is highly stable. A response with a tactile stimulus as conditioned excitant will be set off, for example, by a thermal stimulus of 0° C. and by a musical tone, both given for the first time.[1] Bechterew reports the observation of a similar phenomenon in his work on the motor (leg) reflex, Mikhailoff on the hermit-crab and other forms of marine life, and Beritoff on pigeons.

The converse and correlative process is that of differentiation, whereby the active range of stimuli is narrowed down until only the original note, tone or whatever it may be, calls out the conditioned response. Thus ultimately in the experiments to which reference has been made the sound of A natural alone evoked movement of the hand. At this stage no laboratory noise, shout, or other stimulus had the slightest effect. Similarly Pavlov's dogs might respond to the active tone, and be completely indifferent to one which was an eighth of a tone lower or higher. They might respond originally to an object rotating in either direction but come finally to respond only when clockwise motion was used; or originally to either a square and a circle, finally to a circle only. Differentiation between lights of various colours is reported by Mikhailoff (1920) on the hermit-crab. Beritoff (1926) who used the defence reflex in pigeons reports that it was difficult to establish, the difference between two stimuli not being appreciated after months of work. As many as five hundred repetitions may be without effect. He reports, however, that a reflex built up to noise cannot be elicited with light, and vice versa, and was able to note certain differences between the "individual" responses produced by different organ tones. The feeding reflex is easily differentiated, He feels himself justified in giving differentiation as a characteristic of the conditioned response in the birds used by him.

In general the process of differentiation is often irregular and thus very discouraging to those who are attempting such an experiment for the first time. Further, at a certain stage the inactive stimulus may produce a more extensive response than does the active one. Thus one of Pavlov's examples shows rotation in a clockwise direction as the active stimulus being differentiated from one turning the opposite way. The active stimulus originally called forth saliva corresponding to

[1] Pavlov, 1927, Section VII.

twenty-seven divisions on the apparatus, the inactive seven such divisions. During succeeding experiments the latter figure increased to ten, twelve, and thirty-four such divisions, when it began to decline and finally reached zero. At the same time the figure for the active stimulus never exceeded the original twenty-seven (1927, p. 119). This fluctuation is very characteristic of the process of differentiation and also of the related processes of extinction and negative adaptation. This may be seen for one instance of negative adaptation by reference to Fig. 3, p. 141. The phenomenon is held by Pavlov

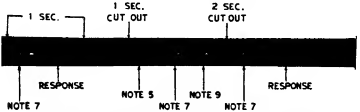


FIG. 4.—Showing that a Conditioned Reflex does not function if its Stimulus-tone is presented as part of a Melody. See page 237.

By administering an electric shock whenever a certain musical tone was heard, the subject was trained to raise his hand at the sound of this tone. No other tone produced this effect. In the record shown, the active note was number seven on the instrument, which was tuned to the common scale. The stimulus is shown by an oscillation of the lower white line, the response by a downward displacement of the same line. It will be seen that at the beginning of the record note number seven was followed as usual by response. About three seconds later notes numbers five, seven and nine were successively played, the sequence occupying rather less than a second of time. It will be observed that, when thus included in an arpeggio, note number seven is not followed by its usual response. About two and a half seconds later the same note in isolation is again followed by a response. Time above in seconds. In order to compress the record, portions representing intervals of one and two seconds respectively have been cut out of the original tracing at the places indicated. No stimulus mark or reaction of any kind appeared on the part cut out. The instrument was of the xylophone type with metal cylinders, which were not damped after a note was struck, consequently the vibrations from one note persisted during the striking of the next note, as in legato piano playing.

to be due to a certain conflict between the inhibitory and excitatory processes, stimuli near the active one initiating both of these processes, and thus having a conflicting effect

on the organism. As to the physiological mechanism by which differentiation is accomplished, there is considerable debate. Generalization is probably to be considered as due to some form of irradiation or spread of neural activity through structures other than those most intimately connected with the stimulus in question. Without such irradiation, indeed, it is difficult to see how "association" could take place, so that there is a certain sense in which generalization may be said to be a by-product of the conditioning process.

For differentiation two kinds of explanation are possible. Here we have a process whereby certain outlets from a "sensory" to a "motor" area are rendered impassable while others are unaffected or possibly made more passable by the same process. The mechanism whereby one set of outlets is barred off may be passive, the one set of connections being strengthened at the expense of the other, something as one path across a field may become more pronounced while others fade out by lack of use; or the mechanism may be active, as when all paths across a field are definitely and actively blocked save one. The second kind of explanation is given by Pavlov, who believes that the process of differentiation is accomplished by inhibitory processes that bar certain paths and leave others open. He believes that repetition of any stimulus causes a gradually progressive inhibition. Thus he thinks of the repetition of the whole process of establishment of the conditioned reflex as causing inhibition of greater and greater intensity, until it finally renders impassable all save the most direct path; indeed, he claims that repetition will ultimately bar this path as well, so that the conditioned reflex becomes totally inoperative (1929, p. 236). This conception is criticized by Beritoff, who points out that Pavlov's theory involves the special assumption that the centre of excitation causes a state of inhibition to arise round itself, an assumption which, he says, is not true of any other form of excitation. Further, he questions whether the inhibition could last as long or whether it could spread as slowly as Pavlov's theory requires. In place of active inhibition as a physiological cause of differentiation he substituted a general lowering of excitability, particularly round the points excited by the active stimulus. According to his theory differentiation is a relatively passive process. It must be confessed that neither of these explanations give a clear picture of the mechanism of

this important phenomenon. Pavlov's explanation is perhaps the more elaborately worked out, but Beritoff's objections must be allowed to have considerable weight. It is possible that a more adequate cerebral neurology will succeed in combining the two principles.

Whatever its mechanism, the process of differentiation, like that of extinction, shows the tendency of the organism to revert to a "normal" state. In the case of extinction and negative adaptation we have the gradual assumption of a state of indifference to an excitant, internal organic changes resulting in a conservative equilibrium towards a series of stimuli. The progress of differentiation seems to exhibit a conflict between the tendency to achieve such a constant state of indifference and the tendency towards excitation. As we have seen, the total series of double stimuli, comprising, for example, food and sound, has been so timed that reaction consistently takes place to the food. After differentiation is completed certain fresh stimuli do indeed cause the salivary reaction, but their number has been reduced. To most of the stimuli originally causing the salivary reaction the organism has achieved such a state of indifference; it may be said that indifference has been reached as far as is possible under the given conditions. Under the influence of the two tendencies, that towards excitation and that towards a state of indifference, the organism comes during the process of differentiation to a fresh equilibrium or constant state. Each process spreads until it can extend no farther owing to the restrictions imposed by the other. The position of balance between the two tendencies is thus, like other limits, expressible in maximal or minimal terms. There is a maximum possible spread of the checking process, whatever it is, consistent with the periodic state of excitation initiated by the conditioned or secondary stimulus. A minimal number of exciting stimuli are left. In fact, another and perfectly legitimate way of expressing the facts of the process of differentiation would be to say that the organism has so changed as the result of the serial incidence of the two simultaneous stimuli that there occurs the minimum possible salivation under the new circumstances. Such a statement would perhaps accord with Wheeler's principle of least action (Wheeler, 1929) and is consistent with what we find in the more complex maze situation.

The biological importance of generalization and differentiation it is not difficult to see. Pavlov points out, for example, that natural stimuli are not constant but vary round a particular strength and quality of stimulus. "The hostile sound of any beast of prey," he says, "serves as a conditioned stimulus to a defence reflex in the animals which it hunts. The defence reflex is brought about independently of variations in pitch, strength and timbre of the sound produced by the animal according to the distance, the tension of the vocal cords and similar factors." [1] A similar process is characteristic of more complex mental life. An infant may be observed to react in the same way to all men as to his father; he will at first give the name of "dog" or "horse" to many other animals. If he has been bitten by a dog he will cry when any dog approaches, and so on. Subjectively speaking all Chinese or Indians look alike; so do animals, which as the naturalists are fond of insisting, and as every laboratory worker with them knows, soon acquire by familiarity their own very marked individualities. In this original similarity of effect and subsequent distinction of differing stimuli there is a close parallel to Pavlov's generalization and differentiation, and it is probable that the same neural process is responsible for both phenomena; though if this is so the process must occur in a highly simplified form when the dog learns the simple distinctions required by the conditioned reflex experiments. The process offers many similarities to that called by the Gestalt psychologists the sharpening [2] of the configuration, a term descriptive of the fact that a configuration tends towards a minimal or maximal state of structural energy. Strictly speaking the same process gives an instance of the transfer of training. A dog has been trained to respond in a certain way to one stimulus; at the end of the training he responds in the same way to other stimuli, and apparently continues to do so unless these latter are given alone without reinforcement, that is to say, until the animal is taught not to respond in this specific way to the stimuli in question. It is interesting to notice that, according to Beritoff, generalization does not take place in what he calls new homogeneous conditioned reflexes, namely those which are—very easily—formed by the combination of new conditioned stimuli with the same active stimulus as was originally employed.

[1] 1927, p. 113. [2] Prägnanz, Köhler, 1920 (the last chapter).

This statement he claims to be true for both dogs and pigeons.[1]

Time Relations of the Conditioned Reflex.—In his early lectures Pavlov stated quite categorically that the unconditioned stimulus cannot follow the conditioned one in time. The ordinary method is, for example, for the light to be given, and later, before it is extinguished, the food. Thus there is no strict simultaneity but rather an overlapping of the two stimuli. From this arrangement of Pavlov developed his conception of the signal reflex, which regarded the secondary stimulus as the " signal " that the primary stimulus was about to follow, the whole process being termed " signalization ".[2] Later experimenters have succeeded in obtaining reversed conditioned reflexes, where the conditioned stimulus follows the unconditioned one. Successful experiments of this kind are reported to have been made upon dogs in Pavlov's[3] laboratory, by Beritoff[4] on pigeons and by Switzer on the lid reflex[5] and the knee-jerk, though this investigator failed to obtain a reversed psycho-galvanic reflex. Bechterew similarly claims to have established reversed conditioned reflexes, presumably of the motor type and with the human being as subject.[6] The fact that such reactions may be formed seems therefore to be sufficiently established, although little is known of their properties.

Reaction Time and Extent.—The reaction time of the conditioned response presents a somewhat difficult problem. As under ordinary circumstances the conditioned stimulus precedes the unconditioned, it appears to be almost inevitable that reaction time for the conditioned should be longer than that for the unconditioned reflex. Pavlov claims that the latent period of the conditioned response is of the same order as reflex time, and in this he agrees with Cason who worked on the eyelid reaction. The majority of the experiments, however, agree in placing the latency of motor conditioned reflexes within the range for the so-called voluntary reaction,

[1] 1926. Here we have not, of course, "secondary" conditioned reflexes. For these compare Williams quoted by Tolman, 1932, p. 148, where rats learn to run the maze in order to reach not food but a " sign " of food.

[2] 1927, pp. 23, 24.

[3] Croonian Lecture, 1928, also reported in Pavlov, 1927, p. 393. This last reference I missed when preparing my paper (1928).

[4] 1926. [5] 1930. [6] 1926, pp. 253, 254.

which is longer than that for the "unconditioned" reflex proper.[1] Frenzied opposition to the term reflex does not, however, seem to be justified on this account,[2] particularly when Pavlov's original statements are borne in mind.[3] Many writers, however, prefer to use the term conditioned response, and the usage is unobjectionable as long as it is free from any implication that the reaction is any less rigidly determined and thus less amenable to scientific analysis than the simpler type of reaction. It is, of course, admitted on all hands that the nervous connections involved in the conditioned reaction are of much greater complexity, which would make it reasonable to expect a longer latent period, but does not, at the same time, necessitate a qualitative difference.

The question may be raised of the extent of the conditioned, relative to that of the unconditioned, reflex. Certain difficulties seem to be involved. It is well known that the extent of the unconditioned reflex depends on a number of variables, among which is the intensity of the stimulus. The conditioned reflex, of course, necessitates the use of two stimuli, the intensity of each of which may vary, and it is known that a more intense conditioned stimulus may result in a more extensive conditioned response.[4] To the question, therefore, whether the conditioned response is always equal in extent to the unconditioned response, the answer must be no. The amplitude varies during formation, and, after formation, with the intensity of stimulus. Thus, for example, Hilgard's[5] conditioned lid response is of much smaller amplitude than the corresponding unconditioned response, as may be seen by reference to the plate on p. 213. The conditions under which the two responses may be of the same magnitude remain to be answered by experiment.

Inhibition and the Conditioned Response.—In previous discussions both of habituation and of various aspects of

[1] Hilgard, 1931, p. 33.

[2] As in Hamel, 1919.

[3] See Pavlov, 1927, p. 105.

[4] Kupalov, Lyman and Lukov, 1931, who refer to other experiments. Cf. Pavlov, 1927, p. 387. It is important not to commit the "stimulus error" by confusing intensity of stimulus with response.

[5] Hilgard, 1931; cf. also Schlosberg, 1928, p. 485. Examination of any of the published papers where conditioned and unconditioned response are both recorded will show that as a rule conditioned is less extensive than unconditioned response.

the conditioned response considerable use has been made of the term inhibition. By this term has been understood a physiological process actively checking response. No implication is intended as to the nature of the process, nor as to whether there is actually more than one mechanism which may produce the same result. These questions are exhaustively discussed elsewhere, as in Dodge's (1926) and Howell's (1925) excellent and thorough-going papers. At bottom the discussion belongs, of course, to physiology, although, as Dodge points out, psychologists have also a certain responsibility, if not to elucidate the ultimate physics of the phenomenon, at least to see that the psychological use of the term is not inconsistent with that of investigators whose interests lie along different lines. This is exactly where Pavlov's results are so puzzling. In fact they are in certain respects directly contradictory to the findings of other workers.[1]

Pavlov holds that inhibition during the formation of conditioned responses presents many parallels to excitation. His results seem to show that a stimulus originally inactive as regards its inhibitory effect may be conditioned; that is to say, it may be caused to acquire inhibitory properties. Such inhibition will spread, and then concentrate, in the cerebral hemispheres, just as the excitatory process (irradiation). It may be removed by other stimuli, giving disinhibition, originally called inhibition of inhibition. A conditioned inhibitor, once established, will check reactions other than the one with which it was originally associated. As the inhibitory process progresses, sleep and even a hypnotic state gradually develops. An important difference between the inhibitory and the excitatory process is to be seen in the fact that the former is more mobile than the latter, in that it is removed or reversed by less powerful stimuli. This is no place for a full description of these highly important results, nor for their neurological criticism. For such a description the reader is referred to Pavlov's own account, and for a thorough-going criticism to Beritoff's papers (1924, 1926, 1927). It may, however, be said that peculiar difficulties attend the idea of the spread of inhibition and of its transferability. That the inhibitory process is intimately connected with that of excitation is undoubted; for an understanding of the exact nature of this connection we must wait at least until the puzzle

[1] See *e.g.* Beritoff, 1924.

of excitation is itself resolved. Pavlov's findings are apparently undisputed, though in certain responsible quarters doubt has been expressed as to the stringency of the scientific controls employed. Few have indeed the technique to question the facts; their reconciliation with current neurological doctrine is another matter.

As particularly important in connection with the conception of "acquired inhibition" it is worth while to describe in detail one of these phenomena, namely, the formation of conditioned inhibition. The example is taken from Pavlov's work entitled *Conditioned Reflexes.* A metronome has, by association with food, acquired the property of exciting the salivary gland, producing a secretion of 8 to 9 drops. An automobile hooter is now sounded for ten seconds, being followed by a pause also of ten seconds, after which the metronome stimulus is given without being followed by food. At the first combination salivary secretion is produced as usual by the metronome. The second such combination reduces the conditioned response to six divisions on the apparatus, though, seven minutes before, the full response of nine divisions had appeared to the metronome alone. Twenty-four days later the metronome, when preceded by the hooter, produces a response of one division only. The metronome when given by itself was always "reinforced" by food; when preceded by the hooter it was never so "reinforced" (pp. 69, 70). Thus, in Pavlov's language, it served as a "signal" for no food. The process is entirely analogous with "differentiation", of which indeed it is a special case. The "hooter and metronome" combination has really become a new stimulus, and has been differentiated out; just as Pavlov describes the differentiation of musical notes 1, 2, 3, 4 from other permutations such as 4, 3, 2, 1, as being due to the fact that different modifications behave as different stimuli. The formation of the "conditioned inhibition" must be considered to be accompanied by the development of a new neural pattern, involving cortical regions or foci corresponding to the two stimuli in question. The response is determined by the total unitary character of the neural pattern, which latter is not activated by linearly summated "stimuli". There is formed "a very complicated excitatory unit" (Pavlov)—which is not the linear sum of the two stimuli. These results are strongly confirmatory

both of the Gestalt interpretation of neural activity and of such work as Lashley's on the functions of the cerebrum.

Mention has already been made of the fact that such negative conditioning is a highly simplified form of " learning not to do ", or " modification by surrender ", such as was discussed in the last chapter. Such surrender may clearly be of definitely economical value. Self-conservation may be as definitely promoted by economy of energy as by its expenditure. In the more complex organisms the two processes are in practice found to go hand in hand, self-conservation being effected simultaneously by acquisition and by sacrifice of reaction. Such economical expenditure occurs in the formation of a conditioned reflex, where attachment of a new response to a stimulus is accompanied by the surrender of the old response; it is seen in " extinction " where unnecessary, uneconomical response is checked; it is seen in the experiments illustrating " conditioned inhibition ", which give a refinement of the same process. To this interdependence of excitation and inhibition in the learning process we shall return later.

Locus of the Conditioned Reflex.—The school of Pavlov has always claimed that the conditioned reflex has an exclusively cerebral locus. In a recent publication Pavlov himself states that the unconditioned reflexes are functions of the basal ganglia, the work of the cerebral hemispheres being to correlate these unconditioned reflexes with external and internal environmental conditions (1930). It seems to be established that the co-operation of the cerebral hemispheres is necessary for the development of conditioned reflexes in the dog, but it does not, of course, follow that in other nervous systems participation of the cerebral cortex is necessary. In particular it does not follow that organisms lacking such a structure are incapable of forming conditioned responses; this is shown, for example, by Mikhailoff's experiments on the hermit-crab and on similar organisms lacking such a structure. Even when such a structure is present it does not follow that its participation is essential for the formation of conditioned responses. Beritoff, for example, claims that the cerebrum, or more properly its cortex, is not necessary for the formation of such responses in doves, basing his conclusion on the results of extirpation (1926). It may, however, be reasonably expected that the site of the neural processes leading to the formation

of these responses in the human being ordinarily lies in the cerebral cortex.

Neurology of Conditioned Reflex Formation.—Thorough-going discussion of the neurological processes involved in the process of "conditioning" would be out of place in a work of this character, even if it were possible in the present state of knowledge. Such discussion belongs to the specialized sphere of neurology, which is specifically interested in the mechanisms underlying the phenomena studied by psychology. In order, however, to give the picture a certain completeness there will be given a statement of the neurological hypotheses which have been advanced to explain the observational data. It will be realized that we have here to do with hypothesis only, for no theory of nervous action in learning can be said as yet to have acquired more than plausibility.

Pavlov uses the general metaphor of attraction to explain what happens neurologically when the conditioned reflex is formed, the cells which are excited by the unconditioned stimulus becoming the focus of such attraction for nervous impulses aroused by new stimuli. Later impulses tend to follow the same path and thus to form a conditioned response.[1] This explanation of phenomena that have been acclaimed as providing an experimental basis for the doctrine of association of ideas bears a striking parallel to a classical passage in Hume. "Here is a kind of Attraction," Hume says, "which in the mental field will be found to have as extraordinary effects as in the natural."[2] The metaphor is interesting, but it is a metaphor only. Bechterew, whose authority as a neurologist is unquestioned, although one feels at times that he is somewhat prolix in hypothesis,[3] speaks in the same terms, adding that there occurs in the excited regions an output of energy "whereby there arises a certain disturbance of the potential between the unexcited or less excited and the more intensely excited regions, with the result that the excitations flow to the latter from the regions functionally related to them."[4]

[1] 1927, p. 38. One may compare Pavlov, 1926, pp. 211–12, for a further statement.

[2] *Treatise on Human Nature*, Part 1, Section IV.

[3] Cf. Brücke, 1926, 1927, p. 443, who in reviewing the *Reflexologie* speaks of Bechterew's vast erudition and authority as a neurologist, but ridicules the quasi-political premises of the book.

[4] 1926, p. 165.

A variation of the "attraction" theory as set forward by Hume and Pavlov is to be found in the well-known "drainage" theory of MacDougall. MacDougall postulates a specific amount of available neural energy, which, if concentrated along certain paths, will be drawn off from others. The theory explains a large number of facts, among which would clearly be the formation of conditioned responses; but it is regarded as inconsistent with the results of modern neurological and psychological investigations.[1]

Very similar to the drainage hypothesis is the statement which William James made to explain "association", namely that when two neural structures have been active together the one tends on re-arousal to propagate its excitement into the other. James seems indeed to have had a certain sympathy for the "drainage" idea.[2] His neurological statement does not, however, explain why association is largely non-reciprocal, why, for example, the sight of food does not make the dog look towards the spot where he once heard a bell at meal-time.

A widely held theory is that of decreased synaptic resistance.[3] This assumes that the junctions between neural elements are changed by the nervous impulse in such a way that passage of succeeding nervous impulses is facilitated. This is the standard text-book theory. It came as something of a boon to teachers of psychology who were in want of an objective basis for habit, and is probably believed more extensively than any other theory, at least in the class-room. In spite of its distinguished adherents, however, its validity has been seriously assailed in the last few years. It seems unable to show why repetition does not always facilitate action. It cannot explain such results as Professor Lashley's who found, for example, that limbs paralysed during training were used in an adaptive manner after training. Here one set of synapses was trained while the learned action could be effected by means of an entirely different set.[4] There seems, in fact, grave difficulty in explaining by the synapsis-decrement theory the whole class of what have been called flexible reactions where

[1] The pertinent literature is listed in Dodge, 1925.

[2] See Holt, 1931, p. 28.

[3] Cf. Pavlov, 1926, p. 274, 1927, p. 141, who refers the formation of Conditioned Reflexes to the synapsis, without, however, specifically mentioning synaptic resistance.

[4] Lashley, 1924*a*, 1930, cf. 1929, *ad loc.*

the end-state is constant but the preceding terms of the action series vary,[1] as in the case where a dog answers his name by running to his master from any part of the house. An identical set of synapses used on successive occasions, with resistances diminished by the passage of impulses, seems too rigid a conception to serve as a neurological substratum for such action. One should beware, however, of assuming dogmatically that synaptic resistance is not lowered by the passage of impulses. All that can be said at present is that there is at present no direct experimental evidence for such lowered resistance, and that there are experimental results which it cannot explain. In particular, decreased synaptic resistance may possibly play some part in the formation of conditioned reflexes. In view of the facts of habituation, extinction and disappearance of secondary response, it is difficult to believe that this process alone is involved even in this relatively simple form of learning.

Somewhat similar to the hypothesis of lowered synaptic resistance, but with more direct experimental evidence to support it, are the laws of Kappers and Bok, according to which conditions of electrical potential at the synapsis may determine growth of the dendrites of one nerve cell towards the axone of the other, and correspondingly that of the axone towards the dendrites. "Association" is said to be due to the fact that the two nerve cells are physically more closely associated after simultaneous excitation.[2] The theory has the support of Professor Holt.[3] It seems, however, to be open to the general objections to a synaptic theory of learning and specifically to the objections urged against the lowered resistance hypothesis, of which indeed it is a specific form. Further, Lashley has pointed out that it seems inadequate to account for the rapidity with which many associations are formed. Here, again, one should avoid the path of dogmatism. While it is improbable that a topographically closer association of axone and dendrite in the synapsis can account for all cases of learning, and specifically of the conditioned response,

[1] Cf. Lashley, 1930, 1924*a*.

[2] Kappers, A., 1928. Three Lectures. London.

[3] 1931, Ch. IV. Cf. Pavlov, 1927, p. 141, "It can only be suggested that in the future synthesizing activity will be referred to the physiochemical properties of synaptic membranes or anastomosing neurofibrils."

it seems equally improbable, in view of experimental results, that such a process is entirely absent at least in deeply grounded associations. Neurobiotaxis is probably present in learning, but it does not seem to be enough to explain learning.

The fact is that neurology is not at present in a position to give much help to the psychologist in his study of the complexities of organic conduct.[1] The theories to which allusion has briefly been made are all based on the assumption that when the organism performs a learned action on successive occasions the same cerebral cells must participate, or more exactly, that successive performances are mediated by a cerebral track which necessarily involves the same neurons. That is to say, the assumption is made that the function of a specific neural element is in general fixed, like the telephone wires to which the brain is so often compared. Modification of response must then be effected by modification in the fixed neural path. This assumption is beginning to be questioned. It was the product of common sense as applied to eighteenth- and nineteenth-century observation; and common sense, while comforting, often turns out to be wrong. In view of the onslaughts of the Gestalt psychologists and of others of the younger generation, it is possible that the hypothesis of fixed neural pathways will have to be replaced by a picture of the brain as a structure over which there play various patterns of excitation and possibly of inhibition. "The same cells may not twice be called upon to perform the same function. They may be in a fixed anatomical relation to the retina, but the functional organization plays over them just as the pattern of letters plays over the bank of lamps in an electric sign.[2] To pervert Father Knox's Limerick, according to this conception the nervous impulse is "in fact not a tram but a bus". With such a picture certain of Pavlov's results are in accord.[3] For example, he states that if a conditioned reflex to a compound stimulus is established, it can be maintained at full strength, and at the same time its individual components may be converted into negative or inhibitory

[1] At the end of a painstaking review Matthaei (1921, p. 40) concludes that no theory can do more than account for part of the facts of memory.

[2] Lashley, 1930, p. 9.

[3] Pavlov, 1927. See especially the sections on conditioned inhibition and compound stimuli.

stimuli.[1] Conversely, the compound stimulus may be given negative or inhibitory properties, while the components keep a positive effect. Here exactly the same stimulus applied to a sense organ results at one time in reaction and at another time in no reaction, according to the setting in which it occurs. It is difficult, though perhaps possible, to reconcile this result with the doctrine of the fixed path of nervous impulse from receptor to effector. Similar results were obtained when the interval between the stimuli was temporal. The same stimulus might be repeated in various complicated rhythms; or successive combinations might be used of stimuli affecting the same receptor, such combinations consisting, for example, of the notes C D E F of a scale, or of a noise, two tones and a bell; or successive compound stimulations might be employed affecting different receptors. In every case a variation of the rhythm or order of presentation could be differentiated from the original, causing Pavlov to maintain that such a different permutation acted as a different stimulus; when this differentiation was effected the separate notes were found to be without effect. So that here again we have a combination of stimuli which elicits a definite response while its components are entirely without effect. In the same way of a strong and a weak stimulus in an active combination the strong one could be extinguished, leaving the combination still active, thus forming the antithesis of what was called "conditioned inhibition", where one stimulus alone was active, but was inactive when presented with another. In another context Pavlov found that the same point of the cortex may assume either excitatory or inhibitory functions on different occasions, although this rule is apparently not universal.[2] Comparable results were reached by the present writer, who found that a subject who had been trained to respond to a certain note would not do so when this active note formed part of an arpeggio.[3] These results seem to be more simply explained on some such theory as that of Lashley, who conceives action to be due to a total pattern, which need not necessarily involve the same anatomical elements at all times, though under similar conditions there is no reason why it should not do so. According as one or the other pattern is initiated individual cells will assume different functions. "The same cells may not be twice called on to perform the

[1] p. 144. [2] Pavlov, 1927, pp. 147, 228. [3] Fig. 4, p. 198.

same function "—at one time they may be part of a neural system leading to excitation of a given effector, at another as part of one indifferent to it. In the same way it will be remembered that Marina's experiments seemed to show that a definite excitatory-inhibitory pattern could persist at a sub-cerebral level when the excito-inhibitory functions of certain of its neural elements had been interchanged. "All the numerous individual cortical points, each at any definite moment, have some very definite physiological significance, while the whole mosaic of functions is integrated into a complex dynamic system, and perpetually achieves a unification of the individual activities. Every new localized influence playing upon it influences to a greater or less extent the entire system."[1] Pavlov had in mind the assumption of fixed functional paths when he wrote these words, but the passage fits equally well Lashley's description.

That the neural pattern corresponding to the conditioned response must not be thought of as the sum of two patterns corresponding respectively to the conditioned and the unconditioned stimulus, is perhaps obvious from the fact that at least some kind of connection must have been established between two foci. The results of recent experiments seem, however, to force us to the further conclusion that when a conditioned reflex is formed we have a neural structure that is entirely new, one which includes constituent elements corresponding to the two stimuli, but in a modified form.

We may first remind ourselves of Beritoff's statement: "The existence in the cortex of foci of raised excitability, and reciprocity between them owing to stimuli acting simultaneously on them, determines the formation of temporary connections" (1924, p. 144). Such reciprocal interaction means first of all that the secondary, in Beritoff's experiments the auditory, stimulus must affect the neural pattern initiated by the primary stimulus, the shock. This is illustrated by a series of observations. For example, "if a weak electrical stimulus which evokes a slight flexion is applied to the limb, and an electric bell is in addition rung for a few seconds, it frequently happens that a stronger flexion of the limb is obtained. . . . When the bell stops the flexion ceases, and restarts when the bell is again rung" (p. 125). Auditory excitation, supervening on the moto-neural pattern, changes

[1] Pavlov, *op. cit.*, p. 232.

this pattern, so that overt action follows. In the same way a very weak electrical stimulus coming before the auditory stimulus favours the conditioned reaction, and it is shown that this must be due to the action of the electric stimulus on the immediate neural effect of the auditory stimulus. Thus when the conditioned reflex is established the neural effect of the bell has been modified, the neural effect of the shock has been modified, and a new connection has been formed. In other words, an entirely new pattern has come into being.

Perhaps the clearest illustration of the newness of the conditioned reflex neural pattern is to be found in the recent important study of Hilgard (1931). This investigator used more accurate timing and recording methods than had hitherto been employed, availing himself of the beautiful Dodge technique. By appropriate timing of the stimuli he was able to obtain a conditioned motor reflex which appeared before the reaction to the unconditioned stimulus had taken place at all. Noise was used as the biologically adequate stimulus for the lid reflex while a light was the secondary stimulus. At first the light has no effect on the eyelid; light and noise are now presented a quarter of a second apart, and after training there appear two lid responses, one, generally small, the conditioned response, the other the response to the sound. Of these the former persists when the sound is not given. The plate reproduced illustrates the process. Such a conditioned response must be initiated by an entirely new neural pattern, formed by the interaction of two others. Here again Pavlov's results are confirmatory. A casual glance at any of his published results will show that conditioned salivation precedes the response to food, the administration of which is termed " reinforcement ".[1]

Once pointed out, this " newness " of the conditioned reflex seems obvious. Thus it has been mentioned that the conditioned response is not necessarily equal in extent to the unconditioned. In Pavlov's results it is apparently very much smaller (1926, Lecture 3, 1927, p. 51, 52). Hilgard's conditioned eyelid response was on the average smaller and examination

[1] Results confirmatory of Hilgard's are reported by Freeman, 1930 (conditioned galvanic reaction); Wever, 1930 (cat); Upton, 1929 (guinea-pig); Schlosberg, 1928 (knee-jerk). Tolman (1932) reproduces Wever's tracings, showing the same point (p. 328).

of Switzer's results [1] shows the same thing. Schlosberg's tracing similarly shows a much smaller amplitude for one conditioned response. The evidence then appears certain that the conditioned response is not a process of tacking on to one stimulus of the response originally belonging to another; by simultaneous excitation a new pattern has been formed, which activates the primary effector and which can be activated by the secondary receptor. This is not to say that conditioned response may not be as extensive as unconditioned; it rather stresses the fact that it need not necessarily be so, so that the "substitution" theory is clearly inadequate. In "extinction" the fresh pattern is temporarily overlaid or "fatigued" [2] It may be reactivated either by lapse of time, though such reactivation is, according to Beritoff, only partial, or by a fresh double excitation, when reactivation is complete.[3] Its formation appears, if Beritoff is correct, to involve a sensory as well as a motor reorganization, a result which would seem to be consistent with subjective experience. One certainly "sees" a word differently when one knows its meaning; and it is equally natural to suppose that the dog "sees" the light differently when it has become the signal for food. As mediating so-called anticipatory response the new pattern is, of course, "economical", as Pavlov and others (Hull, 1929) have pointed out. When formed it is part of the neural counter-correlate of a new conservative equilibrium which the organism has attained to the double excitation. It is a pattern of processes, and is thus four-dimensional, occurring as a sequence of neural events unified in space-time.

Conditioned Response Formation in Relation to the Living System.—We are now in a position to describe and summarize in terms of systemic action what happens when an organism acquires a conditioned response. Let us suppose that the organism in question is in a situation such as that of Pavlov's

[1] 1930, table 3.

[2] See Beritoff, 1927, pp. 255, 257, who concludes that extinction, brought about either by rapid or slower repetition, is produced by an alteration of the mechanism of temporary connection; he uses also the term "fatigue."

[3] Beritoff, *loc. cit.*, who does not believe that another stimulus really resuscitates the "temporary connection" but rather that it increases excitability. The school of Pavlov claims that in all these cases the inhibitions overlaying the temporary connection are annulled.

special laboratory, where external conditions are kept rigidly constant, salivation, for example, being so controlled that it may not occur for as long as two hours. Apart from neurology, which describes the mechanics of the process, these are the facts of conditioned reflex formation. An organic system, one which exhibits to a high degree the property of conserving its general form and function, is being subjected at successive intervals to a double change of external conditions. Let us consider first the non-alimentary factor of this double change. This is such that if it repeatedly occurred by itself the system would, by means of appropriate internal processes, assume a constant conservative state of zero reaction, in spite of the successive changes of external conditions. To borrow Pavlov's term, which has been adopted elsewhere in this book, a series of auditory stimulations, such as those produced by successive ringings of a bell is a "different stimulus" from a single ringing; habituation or extinction sets in to such a series, and is a different organic "response" from the movement following a single ringing. The other environmental change, namely, presentation of food at the same temporal intervals, is also followed by a constant organic state, namely, the rhythmic secretion of a certain amount of saliva.

What is the result in the organism when the two series of external changes coincide, and food and bell are simultaneously and serially presented? From countless experiments it is known that such double presentation of stimuli is also followed by a constant organic state, a state of reactive equilibrium. The orientation response disappears and a constant secretion of saliva occurs at the appropriate intervals. A new organic equilibrium has been achieved.[1] This constant state or equilibrium may be seen to be economic even when thus cursorily considered. It tends towards the conservation of the organism by reducing action to the minimum consistent with what Köhler might call the topography of the situation. Provided, that is to say, that the necessary food is to be taken and digested, action has been reduced to a minimum by the elimination of the orientation action and the retention of the indispensable salivation. In the Pavlovian experiment being considered additional economy of action is artificially effected by the complete elimination of locomotion,

[1] See Child, 1924, on this organic property of perpetually reaching a new equilibrium.

though not of motor response; it will be seen later that this is a special case. The organism has then, by internal changes, made an economical adjustment to the total Situation in which it finds itself, such total Situation being made up of a series of successive presentations of food and bell. The process of differentiation completes the economical adjustment.

If, now, we consider what has been happening in the animal's nervous system, it has been seen that this adjustment has been effected by the genesis of a fresh neural pattern. Where there were two relatively independent patterns at first, the process of adjustment has been accompanied, and perhaps accomplished, by a real synthesis of such patterns. At first the two stimuli were relatively speaking two separate external events, each calling out its appropriate systematization of nervous impulses. As the series of double excitations progresses a third pattern is formed by the interaction of the two already existing.[1] Experimental results up to the present seem to indicate that the new pattern ordinarily requires both stimuli for full activation, and that, further, the conditioned stimulus alone can also activate it, but generally only in part. The latter result is perhaps not unreasonable, in view of the fact that under these circumstances part only of the determining conditions are present. When such activation by the conditioned stimulus alone occurs, we have the unsupported conditioned reflex, which has been seen to differ in general from the fully supported response. Such an unsupported conditioned reflex is artificial; it is, so to speak, a by-product of the organism's synthesising activity, and soon disappears by extinction if repeated.

This new pattern must be regarded as a fresh organic creation, expressive of the organism's progressive act of vital integration. When formed it functions as a unitary four-dimensional sequence of processes excitatory of the effector mechanism. Its development is progressive with repetition; this may be seen by the gradual increase in amount of salivation reported by those who have used this response,[2] and by the increased frequency of reaction still more generally observed as the experiment progresses. This gradual develop-

[1] See Pavlov, 1927, p. 71, for very similar language. On p. 141 he states that the formation of the conditioned reflex is of course primarily an expression of the synthesising activity.

[2] Kleitman and Crisler, 1927.

ment may be otherwise expressed by saying that the new equilibrium to the two simultaneous stimuli does not take place instantaneously but requires time to develop. In the terms of this book, as the organism progresses along the four-dimensional situation equilibrium is gradually effected. This is ordinarily expressed by saying that repetition facilitates action and is an instance of the generalization called the Law of Exercise. However, when the primary stimulus is removed from the Situation, the bell now being given without the food, equilibrium is disturbed because of change of one of the external factors. A corresponding change in neural pattern takes place. The special pattern initiated by repetition of food and bell disappears, for it was correlated with the process of equilibration to the combined stimuli. A new equilibrium is again established, namely, that to bell alone, and brings its own neural pattern. This is accompanied by a gradual degeneration of the salivary response and it is said that the Law of Exercise is violated.[1] This is true; that the Law of Exercise is honoured equally in the breach as in the observance, the studies of adaptation have shown. As the organism is attaining equilibrium with the environment, repetition may sometimes increase or conserve action, sometimes diminish it, according to the nature of the external change that has caused disturbance or reaction. This statement is again, of course, a description not an explanation. We do not know the mechanism by which equilibrium is attained in the two cases. Neither, however, do we know the mechanism by which the "formation of bonds" is expedited by repetition.[2] The fact seems to be that the Law of Exercise suits only half[3] the cases; both the phenomena which obey and those which contradict it are apparently better described in terms of organic equilibrium.

As the organism lives it effects a progressive four-dimensional integration—that is what is meant by its remaining the same organism; when a conditioned reflex is formed, the organism is so constituted that it can make a single conservative Response

[1] But see Thorndike, E. L., 1930, Cason, 1924. Dunlap has pointed out the possibility of three results following repetition (1928).

[2] Matthaei, *e.g.*, after an exhaustive review of possible physiological theories of "association" concludes that no decision can be made between them without entering into the land of hypothesis (1921, p. 40).

[3] Possibly only a third! See Dunlap, 1928, and Thorndike, *loc. cit.*

to a series of double stimulations, such total Response being constituted of a series of lesser partial responses. At any stage reaction to the "conditioned" [1] stimulus is thus part of the total Response to a total repetitive food-bell Situation stretching into the past, and is consequently modified; just as response to a single letter-sign is modified by inclusion in a larger unit such as a word, or for that matter any fresh context. Such modification, due to inclusion of a stimulus in a four-dimensional stimulus-complex, is the formation of the conditioned reflex.

[1] There must be distinguished (*a*) the total series of say fifty food-bell repetitions with the total corresponding response; (*b*) response to any single ring of the bell; (*c*) response to any single presentation of food. Beritoff's theory demands that (*c*) like (*b*) is modified by inclusion in the larger complex. Cf. also Wendt, *op. cit.*, who found such modification in his (unconditioned) knee-jerk. The relation between the conditioned reflex, as initiated by the conditioned stimulus in isolation, and the reaction to the unconditioned stimulus, as in "reinforcement," is very obscure.

CHAPTER IX

THE PLACE OF THE CONDITIONED REFLEX IN THE THEORY OF LEARNING

" Reflexes are the elemental units in the mechanism of perpetual equilibration." (Pavlov.)

JUST as the " association of ideas " had seemed in the days of a purely introspective and reflective mental science to be a key-conception in the explanation of the mind, so in the days when psychology was beginning to be the study of conduct the conditioned reflex appeared to offer a solid experimental basis for the study of action. We have seen how Pavlov came to the idea of studying the so-called psychic secretion of saliva by the use of objective methods and assumptions strictly parallel to those of the other natural sciences. Such a programme was clearly very attractive to the earlier members of the behaviourist school whose endeavours had been exactly to make this psychological transition to objective methods. The conditioned reflex was thus eagerly accepted and made into an important plank of the new movement.

The situation as it appeared to the early behaviourists was something as follows: Human and animal actions might be divided in Pavlov's terminology into conditioned and unconditioned responses. The latter might be set off by the biologically adequate stimulus alone, the former by any stimulus that had been simultaneously associated with it. Pavlov and the other workers had minutely controlled the stimuli, which were such as a carefully standardized pure tone or colour stimulus. After the conditioning process such a pure tone might be considered to have acquired a new response, namely, that originally set off by the biologically adequate stimulus. Now all psychological acquisition might thus be explained; for the physical sciences had devised methods by which an organism's environment might be

exactly analysed and experimentally reproduced when necessary. "In the psychological laboratory when we are dealing with relatively simple factors . . . and are attempting to isolate their effects . . . we speak of stimuli. . . A situation is, of course, upon final analysis, analysable into a complex group of stimuli. . . . Stimuli must be added to or subtracted from . . ." [1] In this scientifically analysable world the biologically adequate stimuli were accordingly those which set off their response unconditionally; when other stimuli were found to cause response, investigation would show that they had acquired such reaction by the conditioned response mechanism. By thus considering the environment as physically analysable and scientifically reproducible in the form of conditioned or unconditioned stimuli, it seemed that another step had been taken towards the establishment of a psychology which should be comparable in method and exactitude to the physical sciences.

Many *ad hoc* psychological terms could apparently be redefined by means of the new concept. Thus, "Habit is a series of conditioned reflexes. The conditioned reflexes are the units into which all habits may be resolved." [2] In the same way for Pavlov the conditioned reflex is the signalling reflex. Under natural conditions the animal must respond not only to directly harmful or beneficial stimuli, but to other physical or chemical agencies, waves of sound, light, and the like, which in themselves only signal the approach of these stimuli. Such phenomena as reaction to a drawing, with its areas of light and shade which come to have a vital significance, he describes as due to the formation of conditioned reflexes which are continually reinforced by tactile and muscular stimuli.[3] For him also "Habits based on training, education and discipline of any sort are nothing but a long chain of conditioned reflexes." [4] Speech is to be referred to the same mechanism. He adds, however, cautions against too rashly applying the results of animal experiment to the "incalculably complex symptoms observed in man".

The conception has also been used to explain those irregular

[1] Watson, 1924, p. 10, 8.

[2] Watson, quoted by Burnham, 1924, p. 170. Cf. Watson, 1925, pp. 157, 158.

[3] 1927, p. 151.

[4] *Ibid.*, p. 395, and Ch. XXIII, *passim*.

cases of psychological state and acquisition known as abnormal. Thus Pavlov, though with caution, elucidates by its aid certain phenomena of human hypnosis.[1] Suggestion is a simple form of conditioned response. Further, the abnormal acquisitions of hysteria are referred to the same mechanism. Thus Kostyleff states that "*in the majority of cases*, and especially those which are most susceptible of cure, *hysteria reduces to the establishment of abnormal reflexes*".[2] Similarly, Watson, in a series of classical experiments, applied the conditioned reflex rubric to the emotional reactions of children.[3]

In the present opinion of the writer, who at one time shared the enthusiasm for the conditioned reflex as a means of giving an objective description for facts hitherto only subjectively describable,[4] such use of Pavlov's mechanism nevertheless gives a false picture of the complexities of human and animal conduct. It depends on two assumptions. First that the technique of physical science may be employed to exhibit certain uniform relations between organic action and certain features of the environment that are specific in the physical sense. These specific features are the stimuli and the resulting organic actions the "reflexes". The second assumption, intimately related to the first, is that the environment of the organism at any moment may be considered to be composed of such determinate stimuli in summation, together with such equally determinate stimuli as have by association with the first class acquired their properties. The latter assumption implies that when stimuli, conditioned or unconditioned, have been defined, they may be treated—added to, subtracted from, recombined and so on—in the manner of purely physical data and without taking the organism further into account.

These assumptions do not appear to be justified. On the contrary it will appear that an environmental feature that is specific from the point of view of physical science need not necessarily be so in relation to the organism; that is to say, it need not by any means invariably call forth the same response under different circumstances. Consequently a total

[1] 1927, p. 406.

[2] Kostyleff, 1914, p. 78.

[3] Watson, J. B., and Rayner, R., 1920; See, 1921; cf. M. C. Jones, 1931.

[4] See, *e.g.*, Humphrey, 1921.

situation cannot be considered to be built up of specific stimuli, each of which may be considered independently of the others in its action on the organism. In fact, psychological analysis and description of a situation is not necessarily best effected by such description and enumeration of elements as would suit the purposes of a physical enquiry.

These statements as here made perhaps assume a certain air of platitude. No psychologist, it may be said, explicitly makes such assumptions. Yet examples may readily be given to show that they are really implicit in much psychological thinking.

Remembering Watson's statement that the conditioned reflexes are the units into which all habits may be resolved, consider, as a single example, Köhler's experiment whereby a hen was trained to choose the lighter of two greys. Watson's statement implies that a conditioned reflex has been formed to grey A, let us say. If this means anything, it means that there has been formed a connection between the approach response and the stimulus presented by grey A. Further, if such description is to have any value it must imply that this stimulus will have the same effect in other connections. If, however, the bird is shown grey A and a still lighter shade of the same colour, Köhler has shown that it will reject the previously "conditioned" stimulus and choose the other which it has never previously seen. Now it would be possible to account for this result by a rather elaborate use of Pavlov's conception of inhibition; just as it was possible in pre-Copernican times to account for the motions of the planets by the Ptolemaic system of cycles and epicycles. Such description would save the reflex-conditioned-reflex theory, exactly as the epicycles saved the statement that the sun revolved round the earth, by an unnecessarily cumbrous and fundamentally unnecessary superstructure. To divide up the situation presented to the hen into a stimulus of one shade plus a conditioned stimulus of another shade then seems to be forcing description along preconceived lines. Here it is confusional to adopt as a psychological unit a "stimulus" that may be manipulated in the manner of a purely physical datum —added to and subtracted from—without asking the question whether such recombination will affect the relation of the "stimulus" to the organism. The same point is shown by the facts concerning inhibition that have come out of Pavlov's

researches. The great experimenter's implicit theory is here belied by the facts that he himself has uncovered.

The unsuitability of this kind of analysis may, again, be seen by considering an animal in its native habitat. Threading its way through the forest it avoids this rock, goes round this tree, runs towards this other animal, and so on. When we attempt to describe the animal's behaviour, that is to say, to give an account of the situation-organism relationship, it is not proper to describe the tree as composed of a collocation of green and brown leaves supported by a dark brown aggregate of particles comprising the trunk. The other animal is not in this connection best described as a collection of bodily covering and organs emitting light waves of various wave-length, intensity and distribution, nor its cry as a sequence of pure tones and overtones. Still less would one seeking for convenient description call the "tree" an aggregate of elaborately organized chemical substances such as those of which the plant physiologist speaks, or think of the rock as a collection of calcium carbonate molecules, or the animal's cry as a complex succession of harmonic air-waves. These statements are so obvious that they have a certain grotesque effect. Yet they may reasonably be in order to make clear the fact that the unit of analysis and method of description which may be useful in one line of enquiry, such as physics, may be entirely unsuited to another, such as psychology, and that different units may legitimately be used for different purposes in analysing exactly the same set of data. They provide a *reductio ad absurdum*.

To return to an example already taken, that of the simple gravitational pendulum. According as I am interested in the iron and carbon atoms as such, or in the properties of harmonic motion, my unit of analysis will be very different. Whenever we are investigating natural phenomena we must, in fact, be careful in our analysis to stop at the stage at which the phenomena under investigation are preserved. The point was apparently first made by Leibnitz, who states that in the subdivision of such a substance as a flint it is possible to go a certain distance only without losing the peculiar properties of flint. It is repeated by Maxwell in the *Theory of Heat*. "We do not assert that there is an absolute limit to the divisibility of matter; what we assert is that after we have divided a body into a certain finite number of con-

stituent parts called molecules, then any further division of these molecules will deprive them of the properties which give rise to the phenomena observed in the substance." [1] And yet, of course, for certain studies such analysis would be necessary—as in modern intra-atomic physics.

The Stimulus.—After these preliminary criticisms let us return now to Pavlov's basic experimental method. "The main requirement is for instruments which are capable of producing a perfectly isolated and unvarying stimulus of a definite degree of intensity." [2] In its immediate context this statement is unquestionable. In order to control psychological or other conditions, it is necessary to have the means of reproducing such conditions exactly. Without such power the experimental method is impossible. With the implication, however, that in such isolated stimuli we have the simplest possible unit for dealing with stimulation, a unit which has at the same time preserved all the essential features of that which is being analysed, namely, the stimulating environment, the statement is open to question. That the implication is there seems to be clear from such references as that to "waves of sound, light and the like" [3] which can serve as conditioned stimuli.

Now it is clear at the outset that in an objective programme the conception of necessary determination is fundamental.[4] The question is, necessary determination by *what*? The great prestige of physical science in the nineteenth century led the scientific world to believe that the analysis it gave of the "world" was the "exact" and ultimate—the "real" analysis, and that it could therefore in its exact and ultimate form be employed to give an account of the determination of physiological by environmental events. Thus the prestige of physical science had much to do with the popularity of the "reflex" hypothesis,[5] which we have seen to be the basis of the "conditioned reflex" theory. It was easy for nineteenth-century workers to equate the environmental, causally determining, factors of organic action with the entities with which

[1] Maxwell, *Theory of Heat*, p. 312. [2] 1927, p. 131. [3] *Ibid.*, p. 14.

[4] Whatever the future may bring forth, physiological and psychological science has not at the present time reached the stage where the possibility of an indeterminary, such as is treated by the newer physics, need be taken into account.

[5] Pavlov, 1926, p. 47.

such physical science dealt, thus obtaining the conception of a "stimulus" as a "wave of light" or as a change in illumination. The reflex hypothesis has of late been subject to considerable criticism for its inability to explain many facts of behaviour. At risk of flogging a dead horse there will here be made certain further criticisms which seem particularly germane to our treatment of the organism as a living system.

Relations between Organism and Environment not necessarily Linear.—After there has been attained in a system, whether living or non-living, equilibrium with surrounding conditions the principle of scientific determinacy requires that external conditions and systemic state are so connected that their interdependence may ideally be expressed in the form of a constant set of relations.[1] But relations need not necessarily be linear, two quantities being linearly related when a specific change of the one is always followed by a proportionately equal change in the other. This may easily be seen for a chemical system.

Consider a substance which is split up into two others when light falls upon it, the action being reversible. Imagine a certain quantity of such a substance in solution under a certain illumination. Decomposition will have taken place. As the chemical reaction is reversible this is momentarily offset by synthesis of the primary substance. For any given intensity of illumination these opposing processes come to a position of equilibrium where synthesis and decomposition balance. Thus the system is in equilibrium internally and with the external conditions. When this happens, the state of the system may be found from the formula

where I is the intensity of the light [2] and x the concentration of the decomposition products, K and a being constants. This equation connects the state of the system with the intensity of illumination by an invariable relation, which is

[1] "The analysis of this establishment of equilibrium in the system forms the first task and goal of physiological investigation considered as a true objective enquiry." Pavlov, 1926, p. 10, who is drawing the parallel between living and non-living systems.

[2] In metre candles.

not, however, a linear relation. The same change in systemic state is by no means produced by the same increase of different illuminations. As a matter of fact approximately equal changes of systemic state, as measured by increase of concentration of the two decomposition products, are produced by changes of light intensity varying from about 194 to about 10,000 metre candles,[1] depending on the amount of illumination already present. To repeat, here is a physico-chemical system, the state of which is connected by a constant relation with external conditions; yet in order to produce equal changes in the system very different external changes may be employed.

Let us now consider an organic system. It is not necessary to go far afield. The physico-chemical system just considered is in fact the one assumed by Hecht to exist in the eye, the sensitivity of which to light he has primarily explained, as we have already seen, by showing that there is in all probability a substance which is thus split into two others by light,[2] with the ultimate production of a substance which stimulates the neural transmitting apparatus. Typical of the animals on which these results were primarily obtained was the clam *Mya Arenaria*, which, when its simple photo-receptor is stimulated, responds by drawing in the siphon. This withdrawal is a constant effect. We have then a living system in which the same constant effect requires, as its environmental counterpart, greatly varying degrees of external change. Systemic conditions are correlated by a constant relation or set of relations with external conditions, and yet there is by no means a linear relation between external and systemic change. The same reflex may require on one occasion fifty times as much increase of environmental energy as on another.

The question now arises: what is the stimulus for this withdrawal reflex to light? It is a change of the external conditions affecting the photoreceptor, and initiating the necessary nervous impulses. But, speaking exactly, many different such external changes will have the effect; many will have it for one intensity of light and not for another.

[1] See Hecht, 1929, p. 248. Strictly speaking we have here a partial system, the total system comprising the light and the physico-chemical system being connected by the constant relation $KI = \frac{x^2}{a-x}$

[2] This is, of course, a highly simplified statement. See Hecht, *loc. cit.*

Nor can we take refuge in any such phrase as "*proportional* to the light intensity already present"; the relationship is much more complex. It is indeed difficult to answer in exact terms the question, "What is *the stimulus* to the reflex withdrawal of *Mya Arenaria*?" The answer, indeed, comes out rather lamely as "Any change in illumination that causes the withdrawal", and to it must be added the caution that some changes do not cause reaction at all; [1] some will cause it at certain intensities and not at others, and the "necessary and sufficient" external changes show enormous quantitative variation. This is rather far from an exact scientific statement, and it seems to make the phrase "*the stimulus* for the withdrawal reflex" scientifically somewhat hazy. The instance of the photic reaction of *Mya Arenaria* is only one of many, the principle being implicitly recognized in much physiological and psychological thought.

Non-linear relationship between environmental conditions and systemic condition or change is indeed the rule. The most famous instance is perhaps to be found in the Weber-Fechner law, which correlates sensation with stimulus by a logarithmic relationship, which is again non-linear. As involving "subjective" factors this law cannot, of course, be quoted in support of our general argument, but it has been given an objective statement by Hoagland [2] who shows that the curve which represents the logarithmic relationship is "an approximation to the mid-region of an integral distribution curve relating thresholds of receptors to logarithms of stimulus". Hoagland gives a number of other examples of organism-environment relations, and while indeed the linear type does occur yet it is by no means universal and depends on special conditions. Thus response of earthworms and the concentration of the acid calling it forth stand in a linear relation, this being due to special conditions. Between the angle a of an inclined plane and the angle θ which rats take when they climb up it there is logarithmic relation involving θ and $\sin a$. For tent-worm caterpillars the relation is likewise logarithmic. For fiddler crabs it is linear. $\sin a$ is the gravitational component in the plane so "this is equivalent to saying

[1] *i.e.* those below the "threshold" at any particular intensity. "Threshold" is, of course, a useful description only; it is an abuse to assign to it any real explanatory significance.

[2] 1930, p. 370.

that the angle θ is proportional to the logarithm of the intensity of the force causing orientation". Crozier has shown that these differences are due to the mechanical structure of the organisms involved, so that the effect of gravitation on these animals depends on the special nature of the living system under consideration. In other cases, as in the circus movements of the slug, the effect on the organism is directly proportional to the logarithm of the light stimulus. Enough has then been said to show that while the general scientific assumption is that there exists a constant relation or set of relations between organic state or response and external conditions, yet these relations cannot be assumed to be linear.[1] So that from an exact point of view the phrase "the stimulus" becomes indeterminate in all these cases exactly as in the case of the photic reaction of *Mya Arenaria*.

Consider then such a statement as Pavlov's, that "determinate (bestimmte) stable and inborn reactions of the higher animals to determinate influences of the outer world, reactions which are mediated by the nervous system, have long been a feature of strictly scientific investigation and have received in physiology the designation of reflexes". Or his description of reflexes as "regular causal connections between definite external stimuli acting on the organism and its necessary reflex reactions" (1927, p. 16). It is clear that such a word as determinate, as applied to environmental influence, cannot stand, at least in connection with the phrase "strictly scientific investigation".[2] Speaking most generally no quantitatively constant and thus fully determinate expression can as a rule be given for a "stimulus" considered as the external excitant of a quantitatively determined reflex. We can speak of a constant relation or set of relations between external conditions and systemic state, conceived in the most

[1] *Idem*, p. 360. References will be found in the paper and in Crozier, 1929.

[2] Pavlov, 1926, p. 194. Cf. also *ibid.*, p. 208, where it is stated that the living creature "antwortet mit seinen tatigkeiten nur auf ganz bestimmte erscheinungen der Aussenwelt," and 1927, p. 7—"a stimulus appears to be connected of necessity with a definite response, as cause with effect." Cf. also Bechterew, 1926, "bei den letzeren (unconditioned reflexes) die Reaktion stets auf einen bestimmten Reiz und nach einen bestimmten Schablone in erstaunlich stereotyper Weise und bei Wiederholung der Reize beliebig oft auftritt" (p. 119). This latter half of the sentence is clearly false. Cf. Beritoff, 1927, pp. 249 ff.

general terms, and between respective changes in these; but that is another thing. Considerations of this kind have indeed led to the proposal from authoritative sources to apply the term stimulus not to the environmental factors affecting a receptor but to the primary change produced in the receptor (Parker and Crozier, 1929, p. 352).[1]

The objection immediately occurs that as a matter of fact the reflexes of the biologist's everyday experience are to a high degree determinate reactions to determinate stimuli. This is because of the special nature both of the structures and the mode of stimulation selected. Often a special "spinal" preparation is used. The sensory structure employed is at the beginning of the experiment in approximate equilibrium with the surrounding organic conditions. This equilibrium is disturbed by the electrical stimulus and action follows. If another stimulus follows within the refractory period, presumably no second reaction takes place. As a rule, however, the time interval is longer; restorative processes have had an opportunity to do their work of bringing the receptor and the other structures approximately to the pre-stimulus state, which is one of equilibrium, again with the surrounding conditions. This equilibrium is once more disturbed electrically and so on. Thus we have something the same thing happening as if a clam in total darkness were subjected to an illuminatory stimulus which was then removed and not repeated until dark adaptation had again set in. In this case it would be found that approximately the same intensity of illumination was necessary and sufficient to cause siphon withdrawal. There would be a "determinate, stable and inborn reaction to a determinate influence of the outer world" (Pavlov). This result would be due to the fact that not only does the organism come into equilibrium with external conditions but this equilibrium is conservative, tending to reinstate the organic pre-stimulus condition.

The same thing may be seen if we examine certain other facts connected with habituation. This phenomenon may be correctly described by saying that on repeated stimulation by a constant disturbance the organism gradually approaches a

[1] Cf. the interesting analysis of the reflex concept by Skinner (1931), who maintains that the concept can be useful only in expressing the fact of determinate relationship between organism and environment.

new conservative equilibrium. As we have seen such habituation of reflexes is called by Sherrington "fatigue" [1]; and by Beritoff "extinction". A tracing of Sherrington's shows about seventy reactions of a spinal dog. These are of very unequal extent, gradually becoming slower in rhythm, progressively more irregular and of smaller amplitude.[2] With different experimental conditions Beritoff shows up to seventeen reactions of a spinal dog and ten of an intact animal, all of them very variable in extent.[3] Here then are cases where the same determinate stimulus produces different effects according to its place in the total series of stimuli. The determinate reflex causally produced by a determinate stimulus is here again a special and highly simplified case. The phenomenon observed may better be thought of as a stage in the attainment of organic equilibrium.[4]

The specific unconditioned reflex on which is to be built the conditioned reflex is thus a simplification which, under certain circumstances, will lead to serious misconception. In certain psychological and physiological contexts this has, of course, been recognized. Thus a stimulus of increased intensity is said to be followed by increased extent of reaction, and so on; and the physiologists have exercised themselves to recognize this fact of observation with the all-or-none law which maintains that nervous and other organic elements respond either maximally or not at all. As a matter of fact when a response is being used as the basis for a conditioned reaction special precautions are taken to ensure the required constant relation between stimulus and response. The animal must be "hungry", and by this means a determinate response to a determinate stimulus is very approximately produced. As we have said the unconditioned stimulus is subject to definite conditions. This is indeed stated by Pavlov in one connection (1927, p. 24). Our quarrel is not, of course, with the fact that "reflexes" do appear; it is rather with the notion that as ordinarily defined and used they give the possibility of establishing a scientific psychology resting on a quantitatively exact set of relations between the environment

[1] It will be remembered that the italics are Sherrington's.

[2] 1911, p. 216.

[3] 1927, pp. 251, 250.

[4] For a criticism of the reflex arc concept, see Wheeler, R. H., 1929, p. 483.

and unlearned reactions. Nor do we quarrel with the fact that conditioning takes place, but rather with the assumption that all acquisition may be described as the process whereby any stimulus can come to act as a substitute [1] for certain other definite stimuli which innately produce a definite organic response. Up to the present our criticism has been to the effect that save in special cases the definite stimulus to a definite, innate response is not typical, and exists only when special arrangements are made to secure it.

Now the assumption has hitherto been made that stimulation consisted in the homogeneous excitation of a single receptor field. When there is inhomogeneity in receptoral excitation the problem becomes much more involved. Suppose, for the sake of simplicity, that one area alone of a visual receptor field is excited. Köhler has discussed the physical chemistry of such an occurrence.[2] In general, according to his hypothesis, the whole receptor-area is involved, coming to an electrochemical equilibrium of its own. In this a special part is played by the illuminated area as contrasted with the rest of the field. The physico-chemical discontinuity thus introduced corresponds subjectively to the perception of a figure-on-a-background, a unitary perception which it is psychologically false to analyse into a detached figure and a separate ground. There begins to be a highly complex relation between such a simple retinal excitation pattern, the external conditions calling it forth, the neural impulses which are initiated, and between all of these and the final equilibrium-state of the organism and the movements of reaction by which this is reached. It has been seen that Koffka and others believe that in such a complex mechanism as the human or near-human eye the whole oculo-motor system comes to its own equilibrium, the functioning of individual neural elements being affected by the state of the whole recepto-neural structure in question. The reactive movements to a specific photic stimulus will be affected by many factors, such as the position of the eyes, the intensity of illumination over the rest of the retinal area, and so on. Again the determinate reaction to a stimulus consisting of determinate light waves does not exist.

[1] "Stellvertreter der beständigen Reize." Pavlov, 1926, p. 195. This was written by Pavlov in 1913.

[2] 1920, p. 195 *et seq.*

We are thus brought to the second assumption of conditioned reflex theory, namely, that a situation may be considered as the aggregate of isolated stimuli. Suppose that there are two such spots of retinal excitation. What will be the relation of the general bodily equilibrium then reached, and the movements employed to reach it, to that attained when a single spot is excited, and of these to the stimuli or changes of external energy? At the present time we have to these questions no answer that will directly relate organic state to surrounding energetic conditions. The Gestalt psychologists have accumulated a large amount of evidence tending to show that such inhomogeneity of excitation gives rise to its own characteristic phenomena, analogous to the stresses and strains following inhomogeneous pressure on a mechanical structure. Thus in a classical series of experiments Wertheimer [1] investigated the effect of successive excitation of different retinal areas, using the method of introspection and report of human subjects. Wertheimer's claim is that under certain conditions the unique perception of movement, named by him in order to avoid implications the ϕ phenomenon, arises from such successive excitation of different parts of the human photo-receptor area. Such perception of movement, analogous to that on which the effect of the moving pictures depends, is not due to excitation of one or other retinal area, nor can it be considered as due to the addition of two excitations, considered as linearly summated. It is rather a qualitatively new phenomenon of consciousness, the biological correlative of which is according to Wertheimer a set of physiological processes induced by mutual interaction of two excited neuro-receptor fields. Thus again, if Wertheimer is right, the "addition" of a specific external stimulus gives rise to entirely fresh physiological results.

In the description of such an occurrence the concept of the addition of stimuli is valueless. It is, of course, true that, when the energy emitted from each surface is given separately, that from the two taken together may be found by a process of linear addition. Yet this will not help us in the psychological problem. For such summation treats the emission of light from the two sources as mutually independent physical events, events which are also ideally independent of any

[1] 1925, pp. 1-105.

organism.[1] As strictly physical events, they are, of course, so independent, at least to a very high degree of approximation. But such a purely physical description will not help in the understanding of the organic occurrence, where the effect of each external event is profoundly modified by the presence of the other. Here, then, is a case of heterogeneous retinal stimulation where addition of " stimuli " is inadequate to describe the correlated psychological or physiological events. Further a determinate stimulus will produce entirely different effects according as it has or has not been preceded by another such stimulus. Watson's statement that " stimuli must be added to or subtracted from " cannot be admitted ; a different method of combination must be employed when the rays of light are considered as physical phenomena and when they are considered as being in relation to the organism, that is, as " stimuli ". This may conceivably be implied in Watson's statement, but it is certainly not expressed.

This again is not to say that linear addition of stimuli is never of value as a method of obtaining the combined organic effect of two or more stimuli taken together. Holt [2] indeed quotes Magnus as laying down a general rule for reflexes of posture that " every group of muscles reacts to the algebraic sum of stimuli arising from the labyrinth and neck receptors " and gives references tending to show that the rule may be extended to cover all reflexes. Whether such algebraic or linear summation of reflex stimuli in the cases given is fundamentally justifiable, or whether as a principle of combination it is approximate and applicable only as a limiting case, I am not competent to say. In either event there is a multitude of cases where the method of linear addition of stimuli is valueless and misleading, especially where the complicated distance receptors of the higher organisms are involved.

This is, of course, only a negative statement concerning a much more complex phenomenon. It may be said that

[1] Heisenberg's principle of uncertainty seems to have reduced physics to a position where it is impossible to ascribe fully determinate law to the data of observation. No event described by physics is here strictly independent of observation, a conclusion which Eddington seems to share. Yet it is true that the ideal of science has been to describe events as they are in relation to themselves, apart from their relation to observation or an observer. See Compton, 1931, and Eddington, 1926.

[2] 1931, p. 71.

combination by linear addition of two quantities is a simple instance of the philosopher's "external relation" to which reference has been already made. When we say that since the length of the straight line A is *a*, and that of line B is *b*, therefore the length of the two together is *a* plus *b*, we assume that the two lengths in question are unaltered by the fact of combination. This is the same as to say that in this fresh combination *a* and *b* are externally related. Now the whole impact of the Gestalt attack on the more traditional psychology has been directed upon this point. It has attempted to show that however physical events may be related in themselves, that is to say, when considered from the physicist's point of view, their organic effects are in general internally related. We are not therefore helped in our understanding of their combined effect by any simple process of combination of the physical stimuli. This is sometimes expressed by saying that Gestalt psychology rejects the "constancy" hypothesis which, in behaviouristic terms, is the reflex hypothesis as stated by Pavlov.[1] Thus in Wertheimer's experiment, when the two excited retinal areas are brought into relation, a fresh unity arises, consisting of two terms in internal relation and such that processes in each excited receptor area are altered in a highly complex manner by being brought into relation with those in the other. Wertheimer's experiments were elaborated by Korte and Koffka, who formulated definite laws for the phenomena he describes.[2] If, then, the stimulus value of an external event is changed by its relation to other stimulating events, each stimulus must be considered in its context and in relation to all other stimuli present. The total external field must be taken into account when considering the stimulus value of any particular physical event. We may again use

[1] In an ordinary chemical calculation the principle of the conservation of matter requires that if the mass of two separate reagents is respectively *a* and *b* the total mass after reaction is *a* plus *b*; that is to say, this mass is found by linear addition of the quantities expressing the separate, unconnected masses. In the same way, under the older physics two velocities were compounded by linear addition of vectors; here as in many other instances modern thought has rejected the process of simple linear addition as a principle of combination. In order accurately to compound two velocities the Lorentz transformation must be employed. Linear addition here gives results which are correct only approximately and in the limiting case.

[2] Korte, A., and Koffka, K., 1915.

an instance already given. In Köhler's experiment in which he trained hens and chimpanzees to choose the lighter of two greys the " stimulus value " of the grey in the first experiment depended upon the total stimulus field; when this was appropriately changed the stimulating effect of a certain shade of grey was profoundly modified, although no alteration had taken place in the stimulating light waves in question. That is to say in the two sets of light waves corresponding to the two shades of grey present at any one experiment there were two external events capable of stimulating the organism; as physical events they would, when treated by the ordinary principles of elementary science, be considered as externally related, each unaffected by the mutual presence of the other. Their organic effects, however, are mutually altered each by the presence of the other.

The situation here considered is, of course, for experimental purposes highly simplified. The general principle must be extended to include the total situation surrounding the organism, including, in the higher forms, very complex visual, auditory and olfactory fields, each of which will comprise a nexus of elements which are relatively independent from the point of view of physical science, but the effects of which on the organism are mutually interconnected in a highly complicated manner. These complications will make necessary a fresh grouping of the physical situation-elements if the uniformities necessary for psychological science are to be exhibited.

There is thus opened up a vista of complexity which far surpasses the conception of a situation which can for psychological purposes be directly analysed by elementary physical treatment into separate stimuli, a situation which is " on final analysis analysable into a complex group of stimuli ". Such analysis will literally lose the wood for the trees. It will destroy the specific organic effects of that which is being analysed. The physical and psychological sciences require different units of analysis in order to exhibit that constancy of relation which is necessary for scientific statement.[1]

The Conditioned Reflex and Habit.—Let us now consider Watson's statement that when a complicated habit is

[1] The point is at bottom the same as that made by Professor Köhler in his *Gestalt Psychology* (1929). It was made also by the present writer in a paper written in 1925, *q.v.*

formed each part of the habit is a conditioned reflex.[1] Returning once more to Köhler's experiment. We have seen that the conditioned reflex to the lighter grey is not active when a still lighter grey is presented. So that the psychological situation cannot with any value be analysed into one (conditioned) shade of grey plus another shade.

Thus it can hardly be said that each part of the habit is a conditioned reflex. For if again we say that choosing the lighter grey is a conditioned reflex, then we must qualify this statement by adding that this is such that it functions whenever there is present a darker grey but is inhibited whenever there is a lighter grey. In the same way, if we say that rejecting the darker grey is a conditioned reflex, we must further add that this reflex is such that it is inhibited and a positive conditioned response induced by the presence of a still darker grey. Further one must add that this latter conditioned reflex functions the first time the specific stimulus is presented. In fact, in order to analyse this habit according to Watson's rule we must assume a conditioned reflex that is inhibited by a whole range of stimuli, and at the same time a series of other conditioned reflexes that spring into being at the first presentation of the stimulus, possibly by the mechanism of generalization. These qualifications are felt to be grotesque, because it is impossible to describe this simple habit in terms of stable conditioned reflexes to fixed stimuli composing the situation. In fact, if a habit or constant response is described in relation to the situation calling it forth, to divide the action into specific conditioned reflexes to specific stimuli makes necessary so many special qualifications that the description becomes useless, just as to describe the planets as rotating round the world in cycles and epicycles is, in practice, useless. Order is only introduced into the description of the simple habit described by saying that the hens have learned to choose that one of two stimuli which is lighter, apart from its absolute degree of lightness, a description in terms of the whole context and one which is fundamentally irreconcilable with the concept of the conditioned response to a specific stimulus.

Music, Reading and the Conditioned Response.—The question under consideration is so important that yet another example

[1] 1925, p. 157. Cf. his analysis of how we may learn to walk round a room by four conditioned reflexes. See also Pavlov, 1927, p. 395.

may be given. Subjects were trained by the writer to raise their hand at the sound of a certain specific tone. The training was accomplished by administering an electric shock when the tone was sounded, but never when any other tone was sounded. Suppose that the active tone was G above middle C. We have then a conditioned reflex to this tone, which has been differentiated out so that no other tone producible on the apparatus was followed by response. By prolonged practice this conditioned reflex became very highly stabilized. Suppose now that the active note is included in a melody or an arpeggio such as that formed by the successive notes C, E, G, C, where G is the active note. The arpeggio then contains the stimulus for the conditioned response. The records show that while the isolated note was consistently followed by response, the same note, repeated immediately in an arpeggio, was consistently not followed by response. The melody " Home Sweet Home " when played in the key of C contains the note G fourteen times.[1] Experiments showed that subjects trained to this note consistently respond to it when presented in isolation and do not respond to it when presented in the melody.[2] This result is very striking in view of the fact of the fourteen repetitions of the active note included in the melody as played. In the same way a scale containing the critical note is regularly not followed by a response, although this note is shown by control experiments to be active. See the tracing on page 198.

If now the arpeggio in relation to the organism is analysable into four notes, successively and linearly[3] added, that is to say, if the arpeggio consists merely of four successive notes of which the active note is one, then there can be no reason why response should not take place in as well as out of the arpeggio. If the melody considered in relation to the organism was nothing but fifty-two successive notes, fourteen of which were the active one, there is no reason why response should not have taken place when the melody was played. The

[1] A slightly simplified form of the melody was used.

[2] Humphrey, 1927. There were one or two minor exceptions to this statement which tend, however, in the direction of "proving the rule." See the original paper.

[3] The word is here used by a slight extension of its mathematical meaning. Strictly speaking linear addition is arithmetical addition, which is, of course, impossible in the case of different notes.

above results show, however, that in finding the organic, psychological effect of the total aggregate of stimuli some other method than that of simple combination of stimuli must be employed. The arpeggio cannot be considered as consisting of three notes with zero effect and one note with active conditioned effect upon the organism. The organic effect of each note, considered as a series of sound waves, is altered by its relation with the other notes. That is to say again, in philosophic language, phenomenally the melody consists of a unity of internally related notes; behaviouristically the stimulus value of the notes composing the melody are internally related, and it is just these internal relations that give the subjective totality the character of a melody or musical phrase. Finally, from the point of view of physics, the melody is a succession of relatively independent sound waves. When, then, Watson asks, "Isn't typewriting, piano-playing and every other act of skill resolvable or analysable into just such a set of units?" [1] that is to say, individual conditioned reflexes to each note, the evidence is very strong that the answer should be no, for the subjects in the above experiment did not react to a melody or musical phrase as to so many discrete notes; and it is safe to say that if they had been playing the melody they would not have done so either.

Here the conditioned reflex enthusiasts, among whom the writer was once numbered, will perhaps answer that in these experiments we have nothing but what might have been expected according to the results of Pavlov's laboratory. The active note has been conditioned by continual association with the electric shock. The other notes have been given an inhibitory value by the process of differentiation. Consequently when the active note occurs in conjunction with the indifferent notes forming the melody, response is inhibited. Thus it would be said that because of this inhibition the arpeggio is a different stimulus from the individual notes composing it, just as Pavlov says that a "compound stimulus, the component units of which remain in themselves unaltered . . . behaves in different modifications as a different stimulus".[2] In fact Ivanov Smolensky in Pavlov's laboratory performed the reverse experiment of the writer's. He established a conditioned reflex to a compound successive stimulus,

[1] 1925, p. 157. [2] 1927, p. 147.

parallel to the four-note arpeggio, and when complete differentiation had taken place found that the individual notes had no effect.[1] But to speak thus of inhibition exerted by the other notes is really to claim exactly what we are claiming. It is to admit that a rational account can only be given of the effect of one conditioned stimulus by considering it in relation to other stimuli. Full scientific description must include the total context, in this case the arpeggio on the background of the experimental room and so on. The only adequate psychological description is to say that, on the common background of the laboratory the isolated note causes a response but the arpeggio containing it does not. Pavlov's statement that external inhibition has occurred because of the other notes is only another way of putting the same thing.[2] Of course, when we say that the arpeggio consists of notes whose stimulus values are internally related, because each is modified by the presence of the other, we are again making the same point as the Gestalt psychologists, who claim that the melody is a unitary configuration; for it has been seen in the second chapter that it is exactly the interrelation of different elements that constitutes them a unity. The supposition that physical events may be linearly combined to form a psychological situation can give no reason why the active tone on one occasion causes response, on another does not do so. The fact is that no analysis of a situation into specific stimuli in the sense of determinate external events correlated with specific conditioned or unconditioned responses can help very much in the description of psychological facts. Response is indeed intimately related to external events, but not in the sense that a determinate "stimulus" from the point of view of physical science is followed by a specific response. The relation between the two is of a much more complex character.

One more example. Consider the activity of reading. Here the pertinent situation is the printed matter which is being read. The obvious element of analysis into which to

[1] 1927, p. 147. Before differentiation of the compound stimulus the individual elements were in general apparently active. See Lecture 7.

[2] It might conceivably be maintained that, because of the process of differentiation, we have here a unique kind of arpeggio. It would probably be hard, however, to find a complex entirely free from the same kind of criticism.

divide this situation is the letter. This was indeed the analysis current thirty years ago. One "learnt one's letters" by a method seemingly akin to the acquisition of conditioned reflexes. The letter was presented and the "name" of it simultaneously given. "A" we were told, was an archer who shot at a frog, and there was the letter A to look at. A line of print would then seem at first sight to offer the ideal possibilities for analysis into discrete stimulus-elements, each of which by training had acquired the property of a conditioned reflex. And yet, as is well known, such analysis is not only fallacious psychologically but harmful pedagogically. Since Cattell's investigation in 1885 we have known that "words and letters are not apperceived singly, one after another, but a whole group together by one mental process".[1] Reaction times to words are but little longer than those to a single letter. In order to "read" the printed page the eye, as is well known, moves in jerks across the line, reading taking place entirely or almost entirely during the periods of rest, during which whole groups of words are apprehended. It is quite clear that the reading situation is not analysable into a number of discrete conditioned stimuli. Considered out of their specific relation with the reacting organism, the lines of print are indeed analysable into a number of linearly additive, previously experienced letters. This is the analysis of the typesetter, who builds up rows of print in the confident expectation that the letters will be unaltered by the fact of their "physical" association. But such typographical analysis and association is of no psychological use. Considered psychologically, in the reading-relation to the organism, the letters are altered by their association with each other in words and sentences. As part determinants of the activity of reading they are not linearly additive, as in the typesetter's "stick". They change their stimulus value according to the particular context. This may further be seen from the fact of proof-readers' error, where the actual perception of a misprint is modified by the context. Typographically, for example, the letters p h i l o s o p b y may be present. Exposure for a short interval in a tachistoscope shows readers reacting as to the word "philosophy" and *seeing* the word philosophy. The stimulus value of the sign "b" has been modified by the context, although typographically it remains the

[1] Quoted by Erdmann and Dodge, 1898, p. 15.

same. In the same way, the two signs i and) have in typewriting an entirely different "meaning" according as they are written together at the beginning of a paragraph as i), or in the directions of an orchestral score as (tutti). Whole groups of letters may thus be completely changed as regards their meaning according to the particular context.

To analyse a reading situation into its typographical letters as component conditioned stimuli would clearly negate the whole purpose of analysis, for it would be to analyse into chaos. It would be comparable to a chemical analysis into elements which were fundamentally changed as chemical elements whenever they entered into a new combination. The reading situation is typical of every other situation which confronts an organism. No situation can be analysed into linearly additive, externally related stimuli, according to the obvious methods of elementary physical science; no habit can be analysed into independent conditioned responses.

Further Objections to the Conditioned Response Theory.—If this criticism of the conditioned response doctrine were not enough, there would yet remain an insuperable objection to the use of Pavlov's mechanism as a unit of learning. The animals on which the experiments were performed were confined in a stall, in which they had been trained to stand quietly. The human beings on which the shock experiments were made were, by the conditions of the work, kept at the table in the obnoxious situation, and, in the writer's experiments, only by means of inducements of one kind or another.[1] Fishes used for the work are tied up, turtles fastened in a clamp. Thus the ordinary conditioned response experiments are in the highest degree unnatural, for by the necessary exigencies of the work the total, normal response of the whole organism is impossible. These total responses ordinarily include locomotion, which is necessarily eliminated in the usual conditioned response work. For example, in describing an experiment that was demonstrated to an audience Pavlov states, " Besides the secretory, the motor component of the

[1] We found considerable difficulty in inducing people to endure the very real discomfort. Students in psychology were willing to lend the *corpus vile* for the sake of science. Others had to be quite highly paid. From personal communication with other experimenters I know that this is not an unusual experience with the shock-conditioning experiment.

food reflex is also very apparent in experiments of this kind. In this very experiment the dog turns in the direction from which it has been customary to present the food and begins to lick its lips vigorously." [1] Anyone who has fed an animal knows that under ordinary, non-experimental conditions the animal would have run towards the food, jumping up to the dish as soon as food or the person bringing it was visible. These intervening motor responses may vary almost infinitely, according to the circumstances and the general topography of the situation. On one occasion a dog or cat may run in one direction round a chair across the kitchen and put his paw on the table where food is being prepared; on another the same animal may follow the food along a totally different route. To call these possibly variant reactions the same constant conditioned reflex to food is to talk scientific nonsense.[2] The official Sovkino film depicts a monkey coming down from a perch to food at the sound of a metronome. There is little doubt that many different series of responses would be used by the animal in this total reaction, all of them initiated by the same "stimulus," and ending with the same "consummatory" response, but including very many different intermediate actions. All of them would be described as conditioned reflexes or as part of the same conditioned reflex. In the same way a cat which has learned to run for food when it hears the plates being removed may be observed to do so wherever it may happen to be at the time. A dog which has learned to answer its name will run to his master on a walk whatever the nature of the intervening terrain. He will, if trained to do so, fetch a cane from land or water. A sheep-dog will "go and fetch the sheep" in whatever part of the field they may be, and so on *ad infinitum*. Pavlov calls the conditioned reflex a signalization response. It is only by eliminating the natural movements of locomotion between the signal and what is signalled that he is able to maintain that the conditioned reflex is parallel to the unconditioned one where "a stimulus appears to be connected of necessity with a definite response". And it is only by limiting his observation to salivation in a confined animal that he is able

[1] 1927, p. 22.

[2] On the date of writing a well-trained rat in a maze in the writer's laboratory took an entirely new "illegitimate" path to food over one of the walls.

to make even reasonable the statement that the animal reacts to the signal in the same way as if it were food, or that no distinction can be observed between the effects produced on the animal by the sounds of the beating metronome and showing it real food.[1] In a natural setting only a very small proportion of the actions produced, for example, in a carnivore by the actual presence of the prey are identical with those produced by the sound of the same animal's cry. The difference is due both to the intervening terrain, which may itself vary indefinitely, and to the different nature of the responses subsequent to salivation. "Signalization" implies a difference in time between the unconditioned and the conditioned reflex.[2] Unless the animal is in the unnaturally confined circumstances of a laboratory, this intervening time will ordinarily be filled with action preceding, and thus additional to, the reaction produced by the unconditioned stimulus. If the intervening reaction between the unconditioned and conditioned stimuli creates a difference, in the same way the final reaction to the unconditioned stimulus is different from that to the conditioned. Nobody will maintain that after hearing the metronome the dog will respond in the same way as to food, by following salivation with the entire normal series of digestive and assimilative mechanisms—a procedure which would involve a spurious digestion followed by a make-believe incorporation of food and ending in a fictitious defecation. One may suspect that even a well-trained dog would find a diet of metronome beats somewhat unsatisfying.

The fact is that a specific conditioned stimulus cannot, as we have already seen, serve in any real way as a substitute for an unconditioned stimulus with a constant, innate response. What we have is a constellation setting off a plastic series of reactions ending, if food be present, in the food activity. This is a very different thing. But when the constancy of the conditioned response is taken away, there disappears also the possibility of the claim that by its means science is able to fulfil the praiseworthy object of establishing the precise relationship existing between the given phenomena of nature and the corresponding activities of the given organism,[3]

[1] *Ibid.*, p. 22.

[2] Even if these overlap, as in the ordinary experiment.

[3] Pavlov, 1926, p. 46.

together with the point of the claim that " habits are nothing but a long chain of conditioned reflexes ". Any satisfactory psychology of habit must take into account the fact that our learned responses are plastic ; any theory which treats habit as a rigid concatenation of specific response loses one of the chief characteristics of habitual actions in the higher organisms. At present science is hardly perhaps in a position to give a full and exact general description [1] of habit in terms of the organism-environment relation. It does not follow that this task is impossible, nor that it may be solved by pretending that the problem is simpler than it really is.

General Statement.—It is now possible to give a general treatment of our primary problem, namely, the position of the conditioned response in learning. The statements that a habit is a series of conditioned reflexes, that the conditioned reflexes are the units into which all habits may be resolved, go with Watson's further statements that " a situation is analysable into a complex group of stimuli " and that " stimuli must be added to or subtracted from ". We have throughout insisted that any part of the universe in which an organism finds itself may be analysed by the technique of physical science into a complex group of physical events. For scientific practice such analysis is essential as providing a means of exactly reproducing and describing objective external events considered as independent of the organism. It has been adapted to the purpose of describing the totality of the objective universe in mutually self-consistent terms. Thus the facts of chemistry have to be described in such a way that all chemical statements are self-consistent ; the facts of combustion must be reconciled with the facts of oxidation in general and these again with chemical combinations of all kinds. Further, the description of the facts of chemistry must be made consistent with the most general facts of physics, such as the laws of thermodynamics, this

[1] Parallel, that is to say, to Crozier's general expression of relationship between the angles θ and α where θ is the angle taken by a rat on a plane inclined at an angle α to the horizon. This relation holds good whatever value α may have. The most highly standardized analysis yet made of habit, namely that reached by the maze experimenters, does not at present approach such generality. It is at present impossible to guess what degree of generality may ultimately be attainable for such an expression. Possibly analysis along Lewin's lines will ultimately be adopted.

being the task of physical chemistry. The ultimate ideal of science is the general description in mutually consistent terms and with as few assumptions as possible of all natural events. Whether or not such should be the ideal is another and irrelevant question. At least the ideal of present-day scientific endeavour is actually an ideal of coherency. When, however, we introduce a special system and describe the world specifically in relation to it, thus deliberately introducing a dichotomy of " inside " and " outside " not present in the most general description, we are at the same time introducing special conditions.[1] For each such system introduced, whether organic or inorganic, we must exhibit special relations designed to take account of the special case thus created. The laws of physics are designed to be perfectly general. If we are describing the effect of external change upon a chemical system, special treatment is necessary, which is not the same as the special treatment necessary for the description of a purely mechanical event, although both must be made consistent with the same fundamental postulates. The task of giving this special treatment, and at the same time of reconciling it with the postulates of science in general, is the task of chemistry and mechanics respectively. In exactly the same way the special task of exhibiting the relation of the external universe to an organic system, and at the same time of reconciling such treatment with the more general postulates of science belongs to the biological and psychological sciences.

Now when the attempt is being made to exhibit the relation of certain events to the organic system, the description appropriate to physical science is not of necessity directly applicable without that special treatment which is forced upon us by the particular task in hand. Stated otherwise, the particular organization of the living system imposes special conditions under which the " laws of nature " are to operate. These special conditions it is the function of psychology and allied sciences to investigate, and in this they join hands with the other special sciences, which have as the subject of investigation their own particular sets of conditions. Much elementary and enthusiastic behaviouristic thinking has lost sight of this fact, and has tacitly assumed that analysis such as is given by physics can be directly applied to exhibit the

[1] Special postulates in the terms of certain logicians.

relation of the world to the organism. In particular this assumption has apparently been responsible for the doctrine that the world may be split up into physical stimuli, " conditioned " and " unconditioned ", which may then be directly manipulated, added to, and subtracted from, in order to exhibit the organism-environment relation.

It would, however, be a mistake to deny, as many have done, the extraordinary value of the work that has come from the laboratories of Pavlov and others who have investigated conditioned reflex. We have seen the premature and illusory nature of the claim that upon the conditioned reflex there can be founded a scientifically determinate psychology parallel to an exact physiology, the basis of which is the unconditioned reflex. This denial of the theoretical conclusions drawn from the results does not impugn the results themselves. Undoubtedly Pavlov's dogs had acquired a habit; after training they habitually salivated at the sound of the bell, just as in the knee-jerk experiments the trained subjects habitually kicked up their foot at the sound of the bell. It is true that the stimulus leading to this habitual action is highly simplified as compared with such a habit as reading, so much so in fact that simplification could hardly go further. After all, environmental change such as the flash of a lamp on a constant background is about as elementary a stimulating situation as may be imagined. But the ensuing reaction is nevertheless habitual. The conditioned reflex, the determinate learned response to a highly simplified and physically determinate environmental event, must indeed be considered as the ultimate word in simplified associational learning. In it, indeed, simplification has gone so far that the habit of everyday life has lost some of its characteristics while retaining others. It has lost the relative robustness of the habit which has given rise to such phrases as " the slave of habit ", a phrase which every psychopathologist knows is often pitiably justified. A large part of the work of many such workers consists exactly in the elimination of habits which, if they were really conditioned reflexes or series of conditioned reflexes, should be with great ease inhibited by repetition of the unreinforced conditioned response. One feels that a dog could never learn to answer its name by means of Pavlov's mechanism, for any slight alteration in the surrounding conditions should cause " external inhibition ",

which might indeed wear off by repetition but should nevertheless be present at each new situation. James' chapter on habit, which is undoubtedly for the most part based on very accurate observation of what actually occurs, forms indeed a striking contrast with Pavlov's conditioned reflex which was put out of operation by every passing cloud.

Habit as we know it outside the laboratory is certainly a much more stable affair than the evanescent conditioned response which, when repeated, progressively and spontaneously disappears.[1] Probably it is best to think of the conditioned reflex as a limiting and an incompletely recorded[2] case of habit, retaining the automaticity and the learned character of the more general mechanism, retaining also the synthesis of neural pattern, whereby, as we shall see, the "association" of stimuli into a complex unity is effected in the more complex forms of learning. In the process, however, of cutting down to the barest minimum the external stimuli by which these results can be effected there has been eliminated the locomotion characteristic of animal as contrasted with plant life; there have also been eliminated, by the sheer simplicity of the stimulation employed, many characteristic receptoral and neural processes; and finally there has been introduced apparently as the result of this simplification a high degree of inhibitability not characteristic of the more complex forms of habitual response.[3] With this special simplified case in mind, the more complex process of maze learning will be considered in the next chapter.

[1] Pavlov, 1927, p. 234. "The cortical cells under the influence of the conditioned stimulus always tend to pass, though sometimes very slowly, into a state of inhibition." Similarly, "A stage is reached when no conditioned secretion can be obtained during any length of isolated action of the conditioned stimulus." The phenomenon is characterized by "invariable progressiveness" (p. 236).

[2] In a personal letter to the writer, Wendt has made the point that the conditioned reflex is incompletely recorded, the whole habit involving, *e.g.*, the salivary secretion to food (reinforcement) as well as that to bell. Wendt prefers not to call the conditioned reflex a simplified form of habit.

[3] In the same way we have special properties belonging to the circle, which is a limiting case of an ellipse; thus the circle, not the ellipse, is the locus of the vertex of triangles with a constant base and a constant opposite angle.

CHAPTER X

THE PROBLEM OF THE MAZE

"Murder would be a prerequisite for the absorption of biology into physics as expressed in the traditional concepts." (Whitehead.)

THE essential principles of the maze method of studying animal learning are too well known to need elaborate description. The experiment consists essentially in placing a hungry or thirsty animal on a number of successive occasions in a labyrinth of the Hampton Court or other type with a food or other reward-chamber attached. The experimenter notes the time taken to reach the "reward" or the numbers of "errors" made, or both, these being found in general to decrease with repetition until a limit is reached, improvement normally being rapid at first and gradually slowing down. After training, in place of a wandering, "curious" animal, requiring anything up to an hour to find the food, one sees an apparently transformed creature, out strictly for business, no sooner placed at the beginning of the first alley-way than it makes for the food-box by the shortest route, disregarding irrelevancies and paying little or no attention to those by-ways so thoroughly explored at first. A dilatory, somewhat busy-bodying amateur has been converted into an animal very much "with a purpose".

In order to understand the objective facts that lie behind this anthropomorphic description, let us imagine such an organism as the rat at rest in relation to the environment, but breathing and performing all the other functions necessary for the maintenance of life. These functions of maintenance are effected through the expenditure of energy. With decrease of the reserve of energy-liberating material we know that hunger contractions are ultimately associated by a mechanism not yet understood in all its details. With the hunger contractions is also associated a greater degree of "restlessness"; the animal is excitable by external stimuli that, after food

has recently been ingested, would cause no visible reaction. This is the specific mechanism whereby internal change in the system disturbs equilibrium with the environment; for, as many writers have pointed out, equilibrium can be disturbed by any alteration in the relation between the organism and its surroundings.[1] Such times of activity are periodic; thus Richter showed that in the case of the white rat the hunger activity occurred at intervals of from one to four hours, according to the conditions of feeding.[2] Habit appears to play a part in determining these periods.

Let us suppose that by some malevolent providence, or what will for the animal come to the same thing, by some benevolent experimenter, such arrangements might conceivably be made that the surrounding topography was perpetually changing. In this chaos the creature's movements will be correspondingly chaotic; his path, for example, during the periods of special activity associated with hunger contractions will not be the same during any two such periods. Suppose that the surroundings are then standardized at regular intervals corresponding to the periods of special activity. This will mean that at the beginning of each activity-period the animal finds himself in the same general surroundings, and that food is to be found at the same place. Under these circumstances locomotion will ordinarily take place under the standardized conditions until food is found. Now such topographical consistency must, from the beginning, impose certain restrictions on action and thus render the successive paths less dissimilar. Under certain conditions we know that the incipient similarity of paths will increase, the successive paths gradually approximating more and more closely to a minimal form until they are indistinguishable from each other. The path taken to food is now approximately constant. At this point a relatively constant state of motion has been reached under the standard and rhythmically recurring conditions.[3] We say that learning has taken place, that the

[1] Cf., for example, Borovski, 1930, p. 50. Verworn and Jennings make the same point, to choose two famous names at random.

[2] 1927, p. 312.

[3] This constant state is, like every other organic phenomenon, relative, not absolute. There seems in many learned performances to be a certain variation round the equilibrium position. See the discussion of the multiple track later in this chapter.

animal has reached his optimal performance for the given situation.

Such standardization of conditions has been attempted in the maze experiments. Non-topographical conditions such as temperature and light are kept as nearly constant as possible from run to run. Topographical conditions are almost exactly constant. If the maze is within the animal's powers the animal's path then gradually approximates a certain form; when the form ceases to change—to improve—we say that the animal has "learned the maze". In general the maze experiment is complicated by the fact that presentations of the maze take place, at least originally, without reference to the hunger contractions and the resulting general activity.

Now it will be remembered that as Dr. Woodger has insisted in his valuable work (1930) we have in the organism to do with a four-dimensional system in a four-dimensional environment. The animal is functioning in three spatial and one temporal dimension all four of which are always involved. It is a complexly unified event, comprising processes which take place in a space-time, from which the temporal and the spatial elements cannot be separated save by a devitalizing abstraction. We cannot think of the organism's life as comprising a series of states, spatial collocations of atoms, muscles, limbs and what not, occurring one after the other in a time dimension like linen on a clothes-line. What we have is a unitary whole process in which time and space are not originally distinguished, a *happening* in time-and-space. The animal is always running in a certain direction at a certain time. He never can run in space pure and simple without time being involved. At a certain time he is lying down and digesting his food; he never does this in a timeless place, any more than he can exist at a placeless time. "The immediate fact for awareness is the whole occurrence of nature. . . . There is no holding nature still and looking at it." [1] In the same way the environment consists of things that are happening. As the animal lives he is confronted with events, manifestations of thermal, photic, chemical energy and so on, as well as that energy of material deformation which gives human beings the conception of solidity in the universe without. These environmental events are likewise taking place in four dimensions which are

[1] Whitehead, 1920, pp. 14, 15.

in experience inseparable. The maze confronts the animal in a certain conformation *at a certain time*; it is not a timeless thing, except by a process really involving abstraction.[1]

The facts of observation in the maze experiments are then that the four-dimensional living system is confronted by a four-dimensional environment with relatively constant features; locomotor disturbance is induced in the system and after a while, according to the exigencies of the situation, a constant state is achieved. This achievement of a relatively constant state, or equilibrium, in the face of a relatively persistent feature of the four-dimensional environment is called learning the maze. Learning is again adjustment to a four-dimensional situation, comprising the successive presentations of the maze at, let us say, twenty-four hour intervals. We may remind ourselves that it is just as legitimate to speak of a total four-dimensional situation comprising a number of such logically distinguishable maze-presentations as it is to consider a printed word as comprising a number of typographically distinguishable letters; even though we ordinarily think of the word as a spatially organized complex of letters, while the total learning-situation before the animal is obviously a spatio-temporal complex of maze presentations.

Once more, if it be said that the word is clearly a single complex, while the successive presentations of the maze are just as clearly separate situations, the answer is twofold. First, the printed word is not clearly a single complex. It consists of elements spatially separated, and it is only in relation to the organism that it is a single complex. As a matter of fact it is generally part of a still more extended complex. Secondly, to say that successive presentations of the maze are clearly separate situations begs the question. We are equally justified in treating a "temporally separated" series of maze presentations as a single complex situation as we are in so treating a "spatially separated" series of carbon marks upon paper. Whether either may profitably be treated as a unit depends entirely on the organism's relation to the series in question. Experiment has amply shown that the word must be treated as a unity that is not the sum of

[1] It is, of course, not maintained that such abstraction is illegitimate; but rather that, for certain purposes, it is unprofitable. By referring to the maze as a repeated situation I am, of course, making it a timeless thing which can be viewed at different times.

its letters. As a matter of fact it is really easier to see that the total series of maze presentations must be considered to form a single four-dimensional situation unity. For the very nature of the learning idea implies that the situations are not separate, discrete, in their effect upon the organism. Since these elements must be grouped together as a whole in order adequately to show the organism-environment relation, clearly the proper way of describing this grouping is by means of the fundamental terms assumed by present-day physical science, which in any case we have no excuse for neglecting in favour of the older set of descriptions.

Now it has already been pointed out that learning of this type is on a par with any response, which involves the disturbance and re-establishment of equilibrium, and which requires special mechanisms both for disturbance and for re-establishment of a constant state. It is on a par also with the single synthetic reaction to a complex situation, such as that involved in reading half a dozen words at a glance, synthesis here being ordinarily thought of as uniting spatially separated elements only, while in maze learning spatio-temporal synthesis is obviously involved. The sole difference between the three cases of adjustment, namely, response to a single stimulus, to a complex one, and learning, is really one of complexity. Except for convenience of description there is no need to speak in the one case of reaction to a situation, in the last of modification by experience, and in the second of integrative reaction.

It has been seen that the organism has the property not only of coming to a constant state, when disturbed by change of conditions, but that this equilibrium is conservative, that is to say that it tends towards the conservation of the biological pattern in question. This is illustrated in maze learning. For on the whole the establishment of the new equilibrium conserves the systemic pattern. It is economical. As tending to conserve the four-dimensional pattern which is life, it is to the "organism's advantage". The organism that can do such a trick has the advantage over one that cannot. As a matter of fact, granting the conception of a system which comes to a new position of equilibrium when disturbed by change of conditions, either external or internal—a property which, given the due presuppositions (p. 33), we have seen to belong to all material systems—and which in addition is so

organized that the fresh equilibrium is also conservative, then such a process as maze learning is a natural development. All stimulus-events are primitively four dimensional. All reaction is four-dimensional organic change in answer to four-dimensional environmental change. It must again be insisted that between a simple reaction, such as a reflex, and a learned response, such as that to a maze, the difference is one of degree only.

The Maze Reaction as a Unified Response.—Now the stimuli employed in the maze experiments may be divided into two classes. There is the reward and the obstacle overcome in order to reach it. Before training has begun, the food and the maze have no relation to each other as stimuli; the maze does not "mean" food to the animal, whatever significance one attaches to the word "mean". After training it is fair to say that the maze means food-at-the-end. The food stimulus and the maze situation have been brought into functional relation with each other, with the result that when the animal has been placed at the entrance of the maze there develops in a progressive and orderly manner a series of actions culminating in the taking of food.

Now learned reaction, like reaction of any kind, requires in such an animal as a rat a definite nervous pattern. Thus on being taken out of the cage a rat trained to run through a certain maze after being inserted through a hole in the glass covering was observed to attempt of its own accord to reach the hole and thus start the run. In order thus to bring the food and the maze into functional relationship as stimuli a fresh neural pattern must have been formed; this is not to be thought of as the sum of two patterns corresponding to "maze" and "food" and still less as the sum of patterns corresponding to the successive stages of the maze—alleys, turns, and so on—plastered fly-paper fashion one after the other with food at the end. This fact is shown rather strikingly by Lashley's recent work. In a well-known series of experiments Lashley trained rats to run a rather simple maze and extirpated various portions of their cerebral cortex. The extirpations differed very widely in position and extent, the latter ranging from 4·5 to 50·9 per cent of the total cortical area. After the operation Lashley found that rats might behave somewhat as they originally did in the maze, moving slowly and exploring; they might wander aimlessly, or repeat

the same error time after time; or they might rush aimlessly at a high speed through the maze. There is no evidence that one part of the habit may be affected without the rest being involved. After injury the habit is affected as a whole, rather than in parts. "None of the studies of learning or retention of the mazes after cerebral lesions has given the slightest indication that the maze habit is made up of independent associational elements. There was never amnesia for one part of the path with retention of another (except that the habit of manipulating the doors, once acquired, was never lost)." [1] The maze habit is a unity. Injury to the cerebral cortex depresses the operation of the whole neural pattern concerned.

Moreover, the habit is such that when the first movements are once initiated the successive reactions follow in an orderly manner without the intervention of the sense organs involved in training. Watson (1914) has shown that external receptors are unnecessary; an animal can apparently run through a complex labyrinth by the use of the kinæsthetic receptors alone. It was thus, as Lashley points out, reasonable to conclude that we have to do with movement-chains each link of which is set off by the preceding link, and which serves in its turn as the stimulus for the next link, the stimulus throughout being kinæsthetic. This hypothesis is furthered by the fact that a trained rat which has been started at a point other than the usual starting-place will make exploratory movements until it has chanced to perform a few correct turns, when it "picks up" the correct procedure and runs the rest of the path correctly. That is, in Washburn's statement, "precisely as playing over the preceding passage would enable a pianist to proceed beyond the point of a breakdown ".[2] In the same way it is found that shortening a passage has for its result that the rats bump up against the end, while lengthening it causes them to attempt a turn at the place where the open passage originally stood.[3]

Yet there is a certain amount of evidence that in the fully formed maze habit we have more than a chain of independent, kinæsthetically initiated single habits. For, to begin with, anyone watching a trained animal can soon convince himself that the run along any passage is influenced by the nature of the succeeding passage. When the animal approaches

[1] Lashley, 1929, p. 141. [2] 1926, p. 289.
[3] Carr and Watson, 1908.

a corner to be turned "he sweeps to the outside and goes around in a wide curve; he does not simply advance step by step to the corner and then start his turn".[1] That is to say, his movements are not linearly or arithmetically additive, but each is affected by the context of the stimulus immediately facing the animal as well as by that stimulus itself, the whole series thus being not a chain of independent links which would be the same were they taken out of the chain; rather there seems to be a unified movement complex, within which the nature of each element is determined by its place in the whole. In other words, the activity of maze running seems to consist of movements that are "internally" related, to use a phrase that has already done hard service. The whole activity seems to be a unity which in a certain sense "determines" the nature of the partial activities according to the topography of the situation and the reactive systems at the animal's disposal. The strategy, so to speak, is fixed, while the tactics may vary; something as the laws of thermodynamics may determine the direction of the flow of heat between two bodies, whatever be the particular circumstances under which this flow takes place.

This concept is illustrated by certain other experimental results. Thus Lashley[2] trained a rat to run a maze that involved turning to the left. After training the animal was subjected to an operation such that a turn in this direction was impossible. On the theory of the kinæsthetic movement-chain the rat should now be unable to traverse the maze. What actually happened was that left-hand turns were accomplished by a three-quarter revolution to the right. As a matter of fact animals may use widely different methods to effect a learned movement, although naturally, in the ordinary conditions of training, very similar means will be adopted on successive similar occasions; just as a human being who has been taught to write with a pen will ordinarily use the finger movements but will nevertheless be able to write on a blackboard without training *ab initio* of the entirely different muscles here involved. So Lashley reports that when animals are trained to run the maze by the "natural" method and are then subjected to various operations, "One drags himself through with his forepaws; another falls at every step but gets through by a series of lunges; a third

[1] Woodworth, 1929, pp. 133, 134. [2] 1921.

rolls over completely in making each turn, yet manages to avoid rolling into a cul-de-sac and makes an errorless run. . . . If the customary sequence of movements employed in reaching the food is rendered impossible, another set, not previously used in the habit, and constituting an entirely different motor pattern, may be directly and efficiently substituted without any random activity." [1] Here we must assume a general unified neural set or pattern directive of activity towards food-in-the-maze. This primary pattern can apparently result in appropriate movement by a number of different mechanisms. A rather pathetic instance of the same kind of thing was observed in the writer's laboratory. A somewhat senescent rat had been trained in a simple maze. On one occasion the animal, being placed at the starting-point, dragged and pushed himself along with his belly touching the ground, stopping at intervals on the way, and thus giving himself kinæsthetic stimulation entirely different from that involved in the usual dapper trot. In this pitiable way a flawless run was made, at the end of which the animal stopped without eating. Within twenty minutes it had died.

By contrast with the theory that such learning takes place by a chain of successively determined, stereotyped actions, even more striking is an experiment of Helson's. Helson attempted to train rats to choose one of a pair of alleys covered with two different shades of grey. Food was placed in both alleys, but was inaccessible in the "wrong" one; both paths included a metal grid, that on the "wrong" side being connected with an electric battery. Thus the choice of one shade of grey was rewarded with food, that of the other was punished by a shock. Now Helson was unable to train the animals to make the discrimination between the two shades. However, after a number of trials one of the rats which had made the wrong choice, instead of subjecting itself to a second shock by retracing its steps over the grid as it had done nine times before, climbed over the partition to the accessible food in the other alley. In a later experiment the same rat jumped directly on the top of the alley-way and thence to the partition separating the alleys and ran to the end, thus obtaining its food without going near the unpleasant grill. "Rat 2 used this method sixteen consecutive times with intervals ranging from twenty-four to forty-eight hours

[1] 1929, p. 137.

between successive sets of experiments and until prevented from climbing by a wire netting placed on the top of the box. When the wire netting covered only the forward portion of the box, to force the rat to choose the grey after getting into the wrong box, instead of going back over the wires it then took the shortest cut to the food by climbing over the partition. . . . When the wire netting was removed this rat resorted to its old method of avoiding the wires. . . ." Of another rat Helson reports that it climbed from either the right or left to the top of the partition in order to do the same thing.[1] Somewhat the same thing was observed, as already reported, in the author's laboratory, where the animal, after thorough training in the maze took an obvious " short cut " by climbing over one of the walls.

These results point to a synthesizing activity in maze learning which is not that of serially fitting one invariable response into another like the lengths of a gas-pipe. One can only interpret the behaviour of Helson's and Lashley's animals by supposing that there has been established a neural pattern which involves, in one whole, patterns corresponding very generally to the maze with its food, and which at the same time can activate a number of different series of interrelated actions with the same final term. Under the accepted neurological theories this supposition is at first sight ridiculous. Yet some such supposition must be made in order to explain what is observed. Moreover, a little reflection will compel us to admit, if we are honest in our psychological thinking, that a very similar hypothesis must be made in connection with many instances of everyday human behaviour. When a mother tells a child to deliver a letter to the neighbour, she expects that the child will go to the back door if no answer is obtained at the front. In the same way a sheep-dog will execute an order, the details of which will vary according to the disposition of the sheep and the nature of the terrain to be traversed. In each of these cases it must be assumed that a neural pattern is formed which includes elements corresponding to certain specific environmental features and which, according to topographical exigencies can determine different series of interrelated responses with the same end-terms.[2] This conclusion is forced on us if we honestly face

[1] Helson, 1927, pp. 393, 394.

[2] The reader is referred to Lashley, 1930, pp. 5 *et seq.*

the facts. The alternative is apparently to believe that somewhere or other the nervous impulses are acted upon by the mind which re-sorts and re-integrates them by some supra-physical means.

Apparently then for such an activity as maze running it is necessary to assume the formation of a neural pattern that is new, that brings into functional relation definite elements corresponding to the maze and the food, and that is capable of initiating different unitary series of motor activities with the same end-term.

Certain other experimental results seem to point in the same direction. There is first the phenomenon of what is called by Tolman the "multiple track".[1] Tolman refers to the experiments of Gilhousen, who found that crayfish, trained to traverse a simple T maze, "tended to indulge in a wide range of varying routes. The animal sometimes followed one side of the stem of the T and sometimes the other."[2] In this connection Tolman quotes also the results of Muenzinger,[3] whose guinea-pigs would use either the left or right paw or the teeth to press a button in order to enter a cage containing food. Rats trained by Yoshioka would choose either of two equally long paths after training. In the same way Dashiell, in a striking series of experiments, found that trained rats would take any one of a number of equally short paths. Dashiell speaks of a "general orientation function, enabling (the rat) to pursue new pathways while remaining successfully oriented towards the objective."[4] This is much the same language as we have used above. We seem then to see in ordinary maze learning the formation of a new neural pattern giving rise to action which is specific only because of the ordinary method of conducting the experiment, but which is naturally flexible. By the conditions of learning an essentially flexible pattern is forced into a more or less rigid mould.[5]

[1] 1932, p. 170. [2] *loc. cit.* [3] 1928.

[4] 1930, p. 139. These examples with others to the same effect are collected by Tolman in his admirable work *Purposive Behaviour in Animals and Man* (Century Co., 1932).

[5] This implies a neural process or pattern which is in a certain sense "prior" to that initiating the learned action on any specific occasion. One may perhaps compare Head's concept of pre-language activity in thought, and Freud's distinction between "latent" and "manifest" dream content. Head, 1926, vol. I.

In this formation of a fresh neural pattern we are reminded of Beritoff's conception of the development in conditioned reflex formation of cortical centres which exert a reciprocal influence upon each other. In fact, bearing in mind the difference of complexity, the conditioned reflex experiments and the maze work point seem to hint at a fundamentally similar neural process, which is in each case the underlying neural process of learning. It is a process of creating a new neural pattern out of the previous *disjecta membra* of originally unassociated patterns. Learning is creation of behaviour and of its underlying neural configuration. This property it shares with all integrative response, that is to say, with all response of an intact organism to an essentially complex world.

The Conditioned Reflex again.—It is then possible to see yet another reason why the conditioned reflex cannot be regarded as the unit of learning. A unit should be the product of an analysis pushed to the furthest point possible without destroying the essential characteristics of that which is being analysed. Thus a molecule is theoretically the smallest particle which retains all the essential properties of the substance analysed. If subdivision is carried further the essential properties of the substance are destroyed. The chemical result of such further subdivision is that the molecule is split up into chemical elements or compounds with different properties. Now by splitting up learning into conditioned reflexes not only have we obtained a very unreliable unit, as we saw in the last chapter, but in eliminating natural locomotion we have gone a long way towards obscuring the effect of the food on the response to the rest of the situation. This effect is very characteristically seen in the maze experiments; in fact learning is here best measured in terms of the improvement of response to the maze situation. It was indeed seen in the last chapter that the formation of a conditioned reflex does affect the original response of the conditioned stimulus; but the effect is masked by the insignificant nature of this response. The characteristic process whereby out of a number of previously unconnected or differently connected responses there is created a new unified response to the maze part of the situation is completely absent in such a rudimentary event as the formation of a conditioned reflex.

There is another characteristic of natural learning which is brought out by the maze experiments, namely, what has been

called the property of plasticity. A learned response is, we have seen, essentially plastic. A conditioned reflex is essentially rigid. Pavlov's purpose in his original experiments was in fact to show that the so-called psychic response was a determinate reaction to a determinate stimulus. The original maze experiment, with its rigid conditions from one trial to another, can indeed be made to exhibit this plasticity. The conditioned reflex experiment, with its specific response to a specific stimulus, and with intervening locomotion cut out, exhibits it not at all. Once more the conditioned reflex cannot be considered as a unit of habit, but as habit simplified in such a way that it has lost some of its characteristic properties. It is, as we saw, only because of this rigidity of the response studied that the "substitution" theory of the conditioned reflex has been possible.

However, the conditioned reflex ideally exhibits the effect of simultaneously working stimuli. True it is that in practice it is found advisable to allow the conditioned stimulus to operate a few seconds before the beginning of the unconditioned one; but it is the overlapping, the simultaneity of the two stimuli that is stressed in the theoretical treatment.[1] Consider now the organism as a four-dimensional continuum faced by a four-dimensional environment. From such a continuum we can in imagination at any moment cut a three-dimensional slice which will represent the animal's position at that instant. An approximation to such a slice would be obtained by making a photograph, or better still, a model of the animal when running the maze. A full representation would show the animal in a position fixed once and for all, amid fixed external and internal conditions. Unfortunately, of course, in order to obtain even an approximation to such a stationary representation we should have to kill the animal. For life is essentially an event in four dimensions. The nearest living representation is obtained in the conditioned reflex experiments, which in their most characteristic form theoretically exhibit the organism as subjected on a number of separate occasions to overlapping stimuli, termed simultaneous, although, of course, time must be introduced in order to show the successive presentations. In this unusual diminishment of the time factor lies the great value of the method. It has shown the mechanism of learning with temporal complications reduced

[1] Pavlov, 1927, p. 26.

to a minimum, and has thus focussed attention on the synthetic aspect of learning. It may perhaps be regarded as a maze experiment with the "reward" moved up to the beginning of the maze, so that the first part of the maze is simultaneous with food, thus cutting out the intervening locomotion, and leaving only the unification or psychological association of the alimentary and non-alimentary parts of the total situation. The delayed and track varieties of conditioned reflex which interpose a period of time between the conditioned and the unconditioned stimulus only make the experiment more artificial by cutting out intervening stimulation. The normal sequence of affairs is for the animal to see or smell his food or his prey, to run for it, and finally to devour it. For him to see it, to wait half a minute in absolute quiet, and then to have food placed in his mouth is, as Pavlov found it to be, a considerable strain on the organism's power of adjustment. It is then not surprising to learn that where food followed a number of seconds after the conditioned stimulus to form a track reflex, synthesis was more difficult to attain and unstable.

Conversely the maze experiment may, again, legitimately be considered as an association experiment, that is to say as one involving the forging of a new unified response to external stimulation originating in more than one receptor field, but with the conditioned stimulus expanded into a highly elaborate spatio-temporal manifold.

The specific problems of the maze thus become those of the establishment of a relatively constant, or equilibrium, state between two four-dimensional manifolds, namely, the highly organized system we call the rat and the less highly organized but still by no means chaotic manifold we describe as the presentation on successive occasions of the spatially identical maze-and-food situation; this latter being organized on a background of environmental flux, much as a word is organized on the neutral background of the paper.

Elimination of Errors.—As in the case of the conditioned reflex, no attempt will be made in this book to treat all the problems and results which have been thrown up by the maze experiments. Many of these have recently been analysed very acutely by Professor Tolman in an authoritative and compendious work.[1] However, there is one problem con-

[1] 1932.

sideration of which is particularly germane to our general treatment. This is the classical puzzle of what is known as the "elimination of errors".

This phrase derives from the "trial and error" theory of learning, which has been so severely criticized by the Gestalt psychologists. Bearing, however, in mind the conception of the establishment of organic equilibrium, let us contrast the final path with any of the earlier ones. Movements have been eliminated, making the final track more economical than the first ones. By such economy the "interest" of the organism has on the whole been furthered. That is to say, the arrival at the learned or "equilibrium" state in relation to successive presentations of food-in-the-maze tends ultimately towards the conservation of the system. The elimination of errors or unnecessary, "useless", movements is then another way of stating the fact that in these more complicated cases the general rule still holds; not *an* equilibrium, but a conservative equilibrium has been reached by the organic system. At bottom an "error" is an unnecessary movement; the fact that the living system achieves a conservative equilibrium towards the situation comprising successive presentations of the maze-food combination really implies then elimination of errors. The advantage in our method of description seems to lie in the fact that it brings the phenomenon in line with many other observed facts of organic life.

But it is possible to consider this development of reaction from another aspect. The most primitive instance of "modification by experience", to use the accepted term, is to be found in the phenomenon comprehensively designated in Chapter VI as habituation, and there discussed under that name. Here we have seen the organism faced by a persistent change of external conditions, gradually coming to a fresh state of equilibrium in which visible motion no longer occurs. Since energy is thus saved, the new equilibrium is a typically biological one. Thus the general principle of the establishment of conservative equilibrium brings complete cessation of motion. Contrast now such entire elimination of external reaction with the conditions in which the organism finds itself when faced by the maze on successive occasions. It is clear that cutting down movement will conduce to organic conservation. It is equally clear, however, that complete

elimination of movement will be unbiological, for it would bring starvation. There must be movement to obtain food.

It is then possible to see two opposing tendencies, each of which must have its effect in the final establishment of relative equilibrium. Organic conservation demands movement-elimination ; organic conservation demands also movement-until-food-is-reached. These demands are both met in the organism capable of synthesizing a unified reaction to such complex conditions. In the maze-running activity of the rat, locomotion is eliminated as far as possible under the condition that the maze must be run to obtain food. This elimination goes on until, under this latter condition, it can go no further ; always bearing in mind that there is required a special organization to effect this result, and leaving out of account " stereotypes " of an uneconomical kind that often persist, presumably because the mechanism is not functioning perfectly. The learned path is thus minimal for the particular topography of the maze. Many writers have pointed this out.[1] The path is minimal because under the given conditions it has been changed as far as possible in the direction of economy, and change in this direction may be subsumed under the general biological principle of Haldane. Such a path possibly represents a minimal expenditure of energy, though there are no experimental data on the point.

In this conception of maze learning as establishing a minimal path because the maximum possible elimination of locomotion has taken place, we have a parallel to the establishment of maxima and minima in many inorganic systems. The stone rolls until, by the total conditions of the gravitational field, movement can go on no longer. " Often ", says Mach when speaking of the establishment of inorganic equilibrium, " the phenomena of nature exhibit maximal or minimal properties because when these greatest or least properties have been established the causes of all further alteration are removed. . . . It sounds much less imposing but is much more elucidatory, much more correct and comprehensive, instead of speaking of the economical tendencies of nature, to say : ' So much

[1] See Szymanski (1917) ; Gengerelli (1930). Bethe noted the point in a famous paper (1898). Helson (1927) also discusses it. Borovski uses fundamentally the same principle in his theory of " adaptive my " (1930).

and so much only occurs as in virtue of the forces and circumstances involved can occur.' "[1]

Granting the particular conditions, first that the animal must eat, secondly that the structure of the maze is such and such, thirdly that the organic system tends towards conservative equilibrium, Mach's words will equally well describe this characteristic phenomenon of maze learning. The animal cannot walk through the wall, must run along the alleys, and so on, and must finally obtain food. There is ultimately established a *dynamic* equilibrium, which involves motion organized in space and time according to the peculiarities of the determining spatio-temporal conditions.[2] The whole process is again very much the same as that of the formation of the conditioned response. In each case movement is cut down as far as possible under the given conditions, and a fresh neural pattern is created involving elements corresponding to the nutritive and non-nutritive parts of the situation.

The actual mechanism of this process of elimination of errors is even more obscure than that of the disappearance of the original response to the conditioned stimulus. The fact to be explained is that the presence of a specific alley is followed by reaction of a certain kind at the beginning of the experiment, and is not so followed at the end. The immediate parallel is of course again the phenomenon of habituation where a specific stimulus is originally followed by a reaction which it finally loses. Possibly the process of habituation is, indeed, involved to a certain extent, in something the same

[1] 1893 (tr.), p. 460.

[2] It must again be insisted that the equilibrium and the minimal path achieved are not in practice absolute. An absolute or ideal equilibrium can never be achieved in concrete cases even by inorganic systems. There are always disturbing forces which cause the observed equilibrium to differ from the ideal, or theoretical one. It must also be remembered that the minimal path is such as can be achieved by the particular organic system in question. Thus when a rat running the maze is observed to turn his head from side to side, we have not maximum possible elimination of motion. To what such residuary movement is due, it is hard at present to say; any statement would be premature. It may be that the animal is not "good enough" to concentrate his attention, in subjective terms, wholly on the job. It may be that waving the head makes running easier for him. It may be that to describe his learning as the achievement of an equilibrium represents only a first approximation to what has happened.

way as it is possibly involved in the extinction of the conditioned reflex.

Apparently we have in negative adaptation, extinction, disappearance of the original response to the conditioned stimulus, and elimination of errors closely allied phenomena. All four are a necessary part of the attainment of new organic equilibrium; in the case of negative adaptation and extinction this equilibrium or constant state is static,[1] while it is dynamic in the disappearance of the original response to the conditioned stimulus and the elimination of maze-errors. For in these last two phenomena after learning has taken place the repeated situation still induces reaction—salivation and maze-running respectively—while at the same time part of the original response has been cut out during the process of learning. Negative adaptation we have attributed to the development of an actively checking process resulting from repetition of the stimulus concerned. Extinction we have found reason to believe somewhat more complex. Here there seems reason to think that, in addition to a checking process due to sheer repetition, repetition of the conditioned without the unconditioned stimulus may also induce some process tending towards degeneration of the conditioned response. In the same way we have seen that it is probably unwise to attribute the degeneration of the original response of the conditioned stimulus to habituation pure and simple. In the discussion of the conditioned reflex we saw that it is highly probable that the synthesis of the new neural pattern may of itself develop some kind of inhibitory or deactivating process which contributes to such phenomena as the disappearance of the "orientation" reflex which have been referred to the drainage hypothesis. This is all more or less speculation. It has, however, a certain amount of experimental background.

Much the same thing can be said of the elimination of errors, for the physiological explanation of which we have, however, no experimental data. Introspective evidence and observation alike seem to show that the development of such a habit as is acquired by the rat may be accompanied by intense "attention" to the relevancies of the situation, with corresponding "distraction" from everything else. It has been pointed out by Sherrington that "attention" implies both activation and inhibition in the central nervous system.[2]

[1] As far as any organic equilibrium can be static. [2] 1911, p. 234.

It is then not unreasonable to believe that the formation of the maze habit implies both the activation of certain nervous patterns and the inhibition of others. Thus a mouse engaged in running a maze may run straight past food left in an unaccustomed place.[1] In view of the intimate relation known to exist in the nervous system between excitation and inhibition the definite establishment of any other conclusion would be surprising.

However, when all is said and done, the part played by inhibition in learning has yet to be described. At the present time we may express ourselves as anticipating that future research will show that the elimination of maze errors is attended by phenomena which are properly described as inhibitory.

Whatever the ultimate solution, the four phenomena to which reference has been made represent four different parts of the same problem. One may suspect that with fuller understanding the solution of the conditioned reflex problems will be seen to be obtainable by simplification of the maze problem. In this way, for example, we can apparently regard the disappearance of the "orientation reflex" as a limiting case of the maximum possible diminution of response to the maze. When the maze-run is zero, maximum possible diminution of response results in zero motion. The circumstances do not demand response, as in the maze proper. In the same way, the habituation experiment will presumably be derived by simplification from the conditioned reflex experiment. For here again response to the *total* situation may economically be entirely eliminated, which is not the case with the conditioned reflex, since the animal must eat.

Of course the foregoing description of maze learning as the establishment of a conservative equilibrium gives no explanation. It merely states that this kind of learning comes under a general descriptive rule which has been seen to hold good of the stimulus-response relation in general. There is no implication that the same mechanism is here involved as in the simpler cases. For all the description tells us, the equilibrium attained by a rat and similar animals in the maze might be reached by an entirely fresh mechanism, a "learning" activity uniquely different from any of the simpler biological processes. If this were so, the description

[1] Unpublished experiments of D. C. G. MacKay.

adopted would still be valuable as showing that learning and the simpler responses are both instances of organic equilibrium, even though this might be attained by different means.

And yet what evidence we have seems to indicate somewhat massively, if generally, that maze and associatory learning in general is not due to a special process which suddenly appears in the evolutionary chain. The point was taken up somewhat extensively in Chapter IV. It was then shown that, starting with the conception of the integrative action of the most elementary organisms—an integration, be it noted, that is effected by a four-dimensional system as the result of four-dimensional stimulation—it is possible to see organic synthesis growing more and more complex as specialization and development proceed, until we come to the complex learned activities of such animals as the rat, which still retain their four-dimensional integrative nature. At no stage, we may repeat, can we say " here learning enters ".[1] Life involves unitary, integrative action. It is apparently because we live at approximately the same tempo as most organic creatures that the learning of a rat seems uniquely different from, let us say, the adaptation of the heart-beat to the exigencies of the body. To an organism the tempo of whose life was such that the fraction of a second's quiescence between heart-beats appeared as long as twenty-four hours to us, the process by which successive beats are related would surely seem to involve some special faculty of memory. And conversely, to a being the tempo of whose life was much slower than ours, the learning of the maze might well seem nothing but a process of almost instantaneous adaptation.

It should further be realized that the total situation involved in learning the maze may include other than repeated and ideally identical presentations of the same maze-food complex. Any particular maze response is effected by an integration which involves a total Situation which has originated in the past. Important elements of this total Situation are the highly similar repetitions of the maze-with-food. Other elements may have involved presentations of maze-features in an entirely different context. This is well shown by the concept of " latent learning ". It is known that an animal which has previously been allowed to " familiarize " itself

[1] Readers of Herbert Spencer will notice that he made somewhat the same point.

with the maze is thereby helped considerably when the maze has to be traversed in order to attain a reward. The fact was established by Lashley and has been corroborated by other workers.[1] It seems to be similar to a very familiar occurrence in human life, where we may use all our "relevant" experience to effect a given task. As we have seen, learning with repetition shades into learning of the "insight" type where repetition is less important and thence into the integration of response to a single complex situation.

We have then a picture of any particular run of the maze as a facet of the progressive integration and perpetual equilibration that is the animal's life. All the past experience of the animal converges on any one run. Reaction is being made to a present element of a total backward-stretching situation which is really co-extensive with the total environment-continuum since birth or even fertilization. On this background of environment-continuum there is organized, as we have seen, the special "learning" situation, including repetitive and non-repetitive elements. This organization takes place because of the four-dimensionally integrative powers of the living system; to say that the living system integrates in four dimensions is to say that it is the same system during the progress of time. The particular system in question can effect this, that, or the other feat of unification according to the efficiency of its mechanism. And finally, when we speak of organization it is not of course implied that anything is done to the things-in-themselves—whatever they are—constituting the environment. The organization is of things-as-they-affect the organism; perhaps we may say, it is an organization of stimuli or environmental features, if we define environment or stimulus in relation to the organism. As Kant pointed out, it is the organism that actively effects the organization. Subjectively, organization is of phenomenal things.[2]

[1] Haney (1931), who refers to the other experiments. See also Tolman, *op. cit.*, on "latent learning". The same fact is illustrated in a more complex form by certain of Maier's experiments (1929).

[2] If it seem to the reader that we are here far from the conceptually simplified systems described in Chapter II, he is reminded that no pretence is made of explaining the living by the non-living system. The discussion of system was for the purpose of illustrating the general meaning of system as a unitary, interrelated complex. The attempt is made only to describe the living system, and that all too roughly.

The Problem of Motive.—The distinction between the alimentary and non-alimentary part of the situation touches the problem of what is ordinarily called "motive", which has already been briefly discussed. It has been seen that when the context of a situation rather than its immediate features seem more important in the determination of action, we ordinarily speak of a motive. We can now go further. There are, we have seen, certain substances in the environment with which the organism stands in a very special relation. These are substances forming food, drink, and so on, such as are necessary for the maintenance of the homœostatic constants. Now if any environmental feature is incorporated in the same context with any of these special substances, then the phenomenon of motive will be especially clear. Context is, of course, a metaphor. But, considering the fact of synthesis in learning as already discussed, we may say that synthesis of neural elements corresponding to both nutritional and non-nutritional features of the environment is the objective basis of what is subjectively known as the motivation of the animal's conduct in the maze. When in Köhler's ingenious experiments the ape was forced to go the long way round in order to obtain his banana, it must be assumed that there was synthesized, this time with relative rapidity, a neural pattern involving elements corresponding to the *détour* and the banana. Or, to take a more complex instance, a child that has learned to avoid a certain street because of a terrifying dog that lives in one of the houses, has presumably effected a neural synthesis comprising elements corresponding to the street and the dog. Equally in these experiments as in the maze-running activities it would ordinarily be said that the "motive" for certain actions is to be found not in the physical conditions immediately confronting the organism but in another part of the total situation. The motive for the ape's *détour* is the banana, that of the child's avoidance of the street is the avoidance of the dog, that of the animal's running the maze is the gaining of the food in the box.

We do not presume to *explain* organic systems by inorganic ones, any more than the chemist presumes to explain logical systems by chemical ones. Living systems are primarily four dimensional. The systems of Chapter II were considered by abstraction to be primarily three dimensional and as exhibiting a related unity that is primarily timeless.

The term meaning is similarly related to the new synthesis of action and neural pattern; the meaning of the maze is now food. Likewise in the conditioned reflex experiment the meaning of the light is now food.

The effect of repetition as such is still obscure.[1] The existence of the refractory and supernormal phases, together with such phenomena as habituation where repetition diminishes overt action, with other cases such as those investigated by Thorndike, where repetition seems to have no effect one way or another, give a picture of astonishing complexity. It seems, however, at least possible to say that in an important group of cases where practice does make perfect we have a gradual synthesis of neural pattern and action providing a motive for action that would otherwise degenerate, and gradually giving meaning to a relatively meaningless complex of stimuli. In many of these cases the synthesis gradually effected involves a feature of the environment with which the organism is in some special relation, such as the chemicals forming food, or air, or even another living system, as in the case of the important but obscure sexual activities.

There are then three main characteristics of the learning process, which may briefly be called the principles of equilibrium, association or synthesis, and minimal reaction. They are three aspects of a single unitary occurrence, and attention has been separately focussed on each of them by experiments on three apparently different forms of modification by experience. These are the habituation, conditioned reflex and maze-running experiments. They are the three modes schematically represented in Chapter V by the action-series of types A, C and C modified. Each of them emphasizes a single aspect of a total four-dimensional event known as learning.

Maze Learning and Organic Equilibrium.—The maze experiments thus provide an admirable example of the complexities of organic equilibrium. To start from first principles, the fact of what is ordinarily called learning forces us to recognize that successive responses to successive maze-presentations are connected. They cannot be regarded as separate, discrete, but must be considered as related to each

[1] Discussions of the "law of exercise" will be found in Thorndike (1931) and Tolman (1932).

other. This is the primary fact from which all discussion of repetitive learning must start. The sum total of all these responses may then be considered as forming a total unified Response, unification being effected by the relations existing between the component responses.[1]

Thus the total behaviour of the animal during twenty experimental sessions may be considered as forming a single Response to the total Situation which is twenty-successive-presentations-of-the-maze. Regarded in this way, improvement of performance is seen as the gradual approach to a position of equilibrium; this represents a stage beyond which improvement cannot go, and is in every way comparable to the organic equilibrium achieved when the organism is confronted by a non-repetitive situation. Such equilibrium is not reached because the organism is *a* system; for we know well that many animals cannot learn the maze. Maze equilibrium is rather reached because the organism is a system constituted in a certain way.

Thus again we must contrast the total Response to twenty-maze-presentations with any single component response, say that reached on the last presentation. At the twentieth presentation the organism is faced with a maze situation the effect of which is different from that of the same situation at the first session. This difference is due to the fact that the maze is now part of a more inclusive, time-extended Situation with its roots in the past; response is modified, because the stimulus has become part of a larger Stimulus complex. Such modification of response is the maze learning. It is sometimes said, as in the case of the conditioned reflex, that the animal anticipates food at the end of the run. This is dangerous language if it implies that he is reacting to a stimulus that has not yet been experienced or which is perhaps not in existence at all. Objectively considered response is progressively called out by environment.[2] When we wish to compare any individual run with a preceding one, then we must take the whole greater Response unity into account. Then it is seen that the individual response to any individual maze presentation is modified because its stimulating situation

[1] Cf. note, p. 8.

[2] There is no attempt, of course, to deny that *subjectively* anticipation may be present. We have specifically excluded the problem of the correlation of "objective" and "subjective" events.

is part of a larger Situation which expands as repetition increases. "I am a part of all that I have met," cried Ulysses. Learned response, like all response, is at any moment a part of the organism's total previous history. Specifically, when repetition has taken place learned reaction to one of the repeated situations is part of the total Response to the repeated situation.

Finally, as to synthesis: the organism is a system the component elements of which are processes. The unifying relations of these constituent elements bind it into what is at any moment a functional unity. This unifying of function is the physiological Integration, and such integration is momentarily progressive. At every instant the organism is effecting progressive integration; if the process stays, even instantaneously, death results.

But here again we are really taking a three-dimensional slice out of biological processes; it is better to consider the whole four-dimensional environment as progressively developing, and, facing it, the four-dimensional organized living system. Then it is seen that if the organism is to remain the same organism throughout its life, integration or unification must be four dimensional. The organism actively effects a spatio-temporal unity; or, in the terms of a preceding chapter, when disturbed it actively effects a conservative equilibrium. Were it not for this four-dimensional unification, this maintenance of itself against a hostile and time-progressing world, the living creature would biologically disintegrate at every instant. When we speak of maze-learning, with its synthesis of progressive presentations of the maze food situation, we are then abstracting for convenience of thought from the total progressive four-dimensional integration that is momentarily being effected. According to its constitution, an organism can effect a more or less complex integration of relatively simultaneous stimuli. Certain organisms can likewise effect a more complex integration or synthesis than can others of situations that are separated in time. That is to say, they can effect the more complex feats of repetitive learning, can better profit by experience.

CHAPTER XI

CONCLUSION

"The word 'memory' is here used in the widest legitimate sense; it implies stability of organization in spite of change, a stability which enables it to survive change and to incorporate effects of change within itself." (William MacDougall.)

THE thesis which this book has attempted to maintain rests upon a truism. It is the truism that if any material complex is found to persist in a world where the flux of Heraclitus reigns continuously, such persistence must ultimately be due to the special organization of the complex. This proposition is as true of inorganic as of organic complexes; equally true of both animate and inanimate nature is the related proposition that in a world where change has reigned continuously in the past those complexes which have such organization-for-constancy will, on the whole, be more in evidence than others lacking such special organization. For they alone survive for any length of time; all others must be disintegrated. These statements are, of course, akin to the affirmation of a doctrine of survival.

While, however, organic and inorganic survival rest on much the same presupposition, yet as we leave the chemical and physical sciences for the biological ones we begin to find a shift of emphasis. The chemical atom is a complex which is highly stable, and which indeed could not until lately be split up by any means at the disposal of physical science. It has its own organization for constancy, the nature of which is, however, not yet known. Thus one is not surprised to find Whitehead maintaining that the atom is an organism—a curious reversal of the older mechanistic point of view. The chemical compounds are similarly systems, interrelated complexes, with organization for constancy, although they are not so stable as the elements. Their organization for constancy is not so robust. Now by far the greatest part of the work done by physicists and chemists upon these stable

systems of inorganic nature has been confined to the investigation of their properties without reference to the possibility of disturbance created by external change. The properties of the carbon atom are examined. We ordinarily see it as stable, not in the process of preserving its stability. If some physicist succeeds in doing the appropriate things to the atom of carbon, we shall see another atom, likewise stable. In the same way, the chemical compounds are in general examined as stable, not in the process of preserving their stability when external conditions change. We know that molecules of water when heated to a high temperature will disintegrate into hydrogen and oxygen. What happens to such a molecule when it is in process of preserving its integrity after external disturbance we do not know, nor, for that matter, whether the phrase has any meaning.

When we come, however, to the biological systems it is of very great importance to observe precisely that which is lost from observation in many pre-biological complexes, namely the displacements which arise after disturbance. Such disturbances are usually said to be due to stimuli, or change of conditions, a usage which affirms belief in the applicability of the '" law of causation " to organic events. Stimuli must, of course, be confined within a limit, which is pathetically narrow if the total possible of cosmic conditions is considered. Change of heat conditions amounting to a few degrees will act as a stimulus for the temperature-regulating and other structures of the body. Just what span of variable heat conditions will so act upon our hothouse human organizations is difficult to say. It is certain that it shrinks into insignificance by contrast with the heat of the stars and the cold of interstellar space. Organic displacements caused by the narrow band of stimulating disturbances we call responses, adjustments, or behaviour. The fact that disturbances never come singly we recognize by the use of the term situation, which denotes a complex of stimuli to which unified organic adjustment is made. Thus in anatomy and physiology the biological sciences investigate the structure by means of which compensatory adjustments are made and life preserved, and to a certain extent the behaviour of the organism after disturbance. Here it is to the genius of such biologists as Sherrington, Adrian, and Forbes that we owe our knowledge of the reflex

and of the nervous impulse, each of them essentially phenomena of disturbance. To the psychologist, however, belongs peculiarly the task of investigating such transitional states between one equilibrium configuration and another. When the four-dimensional organized manifold we call the living system makes an adjustment to a four-dimensional situation containing similar spatio-temporally distributed elements, such as successive presentations of the maze complex, and establishes a new equilibrium to such an extended situation, then we say that learning has taken place. The term is also used by the Gestalt psychologists to describe adjustment, or unified systemic response, to complex situations where the temporal aspect is not so obvious.

There are, of course, many cases where the organism does not learn. Thus it may be impossible for a new equilibrium to be reached to a temporally extended situation which does not include any repetitive features. In an entirely chaotic world, where no two successive events were ever in any way similar, organic equilibrium, at least of the kind seen in repetitive learning, would be impossible for organisms of the type we know. This is because the organism is a partial, not an isolated, system in delicate interrelation with the environment. In such a world there would be no persistent feature of the environment in relation to which such organic equilibrium could itself persist. Subjectively, and by the same token, it would be impossible for the organism to " profit by experience ", since there would be nothing in the past to which the present experience could be referred, for the achievement of a constant state by a system implies a corresponding environmental constancy, whether the system be organic or inorganic.[1] Without this, as we have previously remarked, the constant organic state would be immediately disturbed. In the time-extended biological situation such constancy is given by the repetitive element. Thus it is that from the time when training begins the maze-food complex is experimentally kept as constant as possible, and it is to this complex that the fresh equilibrium is reached. If again the character of the maze were totally altered at each run, the maze-food complex would present to the rat a small

[1] Reservations should perhaps be made as to thresholds. My colleague, Prof. Vlastos, points out that it is doubtful whether absolute chaos is possible.

chaos; once more no equilibrium would be possible, and no learning. Similarly, even if constancy is objectively present in the four-dimensional situation-manifold, the organism may not be sufficiently endowed to achieve the requisite equilibrium. Thus Lashley has shown that a rat deprived of more than 40 per cent. of its cerebral cortex cannot learn the maze. Here we have indeed a biological system, but not one with the organization necessary to achieve equilibrium to such complex environmental conditions. Similarly many intact biological systems of a lowlier order cannot "learn" a maze which presents little difficulty to a rat. A tortoise would be hopelessly lost in a structure that could be learned by a mouse in a dozen trials.

Moreover, a four-dimensional situation presenting chaos to a rat may exhibit to the highly endowed human organism points of similarity which will make learning possible. "The human brain can fabricate symbols and abstractions; it can use language, numbers and equations, design machines, bridges, telescopes and use them. The chimpanzee does not know the meaning of $y^2 = 2px$, *and he can never find out*." [1] The symbols, abstractions and formulæ of science and mathematics are, of course, statements of uniformities in nature, not ultimate explanations. These uniformities can be detected by the more subtle human brain and used to further the ends of human organism. The Egyptian geometer detecting the uniformities in the rise of the Nile waters and using them to ensure next year's crops is doing on an extended scale what the rat does in the maze. Confronted with an originally chaotic spatio-temporal manifold in which to get food and thus to keep intact his bodily organization, he succeeds in economizing time and energy by the recognition of certain similarities in the protean phenomena of nature. Because he can detect these similarities underlying chaos he achieves in his food-hunting activities a constant economical level unknown to his nomadic ancestors. A still more economical and improved level is reached today, as the result of the uniformities detected in nature by agricultural and other sciences. Scientific methods are economical, and economy can proceed only up to a certain point, the point of equilibrium. Scientific formulæ, learned in one connection and with one motive, may later be incorporated into another context by

[1] Herrick, 1926, p. 290.

a process of "latent learning" similar to that observed in the maze-learning of rats.

In any act of learning it has been throughout insisted that we are apt to look not at the total four-dimensional series of actions by which equilibrium is attained, but at the individual actions composing this series. Thus we see a cat escaping from a puzzle box, and cry "See what a clever animal!" The cat appears clever, as Thorndike remarked, because we have not seen the training which preceded the accomplishment. The learned action here appears unique only when we forget that it is a phase of a more extended process of establishing equilibrium. To explain it a unique organic property of memory, mneme, or what not, is often postulated. Thus the cat's performance is said to be possible because the animal "remembers" previous events; or one who is more objectively minded may speak of "organic traces". In each case the statement has about it a certain flavour of the plea of *sui generis* which, however, is not so apparent in the behaviouristic statement. If, however, learning is considered from the point of view here advocated the necessity for any special terminology largely disappears. The rat's learned performance seems qualitatively different from unlearned reactions because, in our inveterate habit of tri-dimensional thought, we are isolating a three-dimensional slice from a four-dimensional event and contrasting it with other, preceding, three-dimensional slices. From first to last the cat's behaviour in the puzzle is a connected, unified process of attaining equilibrium. This at the risk of repetition. If we do not thus consider the total set of reactions in their total context, special terms must be used to explain any individual reaction. In the same way, in order to explain such events as the rise of water in a vacuum without considering the whole relevant content, including the atmospheric pressure, it became necessary to postulate the special principle that nature abhors a vacuum.

Of the peculiar relation of what are called events of consciousness to this process of the attainment of equilibrium, we cannot here speak.[1] The problem of the relation of the events of consciousness to physiological and other "objective"

[1] The reader is referred to the works of Lewin, whose attitude, very much like that of the present work, is entirely subjective. The promised second volume of Holt (1931), is also to discuss the subjective side. See also Tolman (1932).

happenings is an ancient one. It is elsewhere discussed in the light of modern psychological findings. At the beginning of this book permission was asked of the reader to shirk this, the most difficult, if the most fascinating, of all scientific puzzles. Because of this limitation of subject-matter, the discussion of this book is essentially incomplete. It is, however, only by carrying the analysis of "bodily" activities to the furthest possible limit and by the use of the fullest possible objective experimental evidence that the stage may adequately be cleared for the major investigation.

Thus this book ends with a recognition of its incompleteness. Nor is this the full tally. Many of the problems involved in the stupendously complex activity of human learning have not been considered at all. Such are the relation of many of the everyday activities of human life to the major drives or urges; the intricate interlacement of the sexual urge with apparently non-sexual functions; the widely ramifying æsthetic response, the emotional responses. But the enumeration would go on to the crack of doom. Certain of these problems may possibly in the future be found amenable to description along the lines of this book. Some may not be amenable to objective description at all. Others will possibly require an entirely different, though still objective, formulation. For the reader may once more be reminded that in considering the organism as a living system final description was not intended, any more than such completeness of description is implied by the chemist when he shows chemical processes to take place in the organism. In each case the aim is more modestly to provide a slightly more adequate account of certain features of organic behaviour by relating apparently unique organic events to already known and more general principles.

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